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ENDEAVOUR

A quarterly review designed to record the progress of the sciences in the service of mankind

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VOLUME II JANUARY 1943 NUMBER 5

Science and politics

In a book reviewed elsewhere in these columns, the thesis is postulated that 'there is only one aspect of politics which directly concerns scientists as such'—namely their freedom to pursue their own research. The belief that this freedom is of basic importance has already been expressed in ENDEAVOUR, and signs are not wanting that scientists are alive to the dangers of control as opposed to co-ordination. But to asseverate that, except in this respect, scientists should shun politics is to adopt a position wholly indefensible in the light of past events and future prospects. It rests on the tacit but unfounded assumptions that the sciences are extraneous to the rest of human activity and that there are wide regions in which scientific methods are not, and never will be, applicable.

Such assumptions are both pusillanimous and pessimistic. If there is one thing which science has unmistakably demonstrated, it is that the whole of the phenomenal world, including human nature, may confidently be expected to show itself rational in character and homogeneous with those parts of it—still comparatively few—which have already been investigated by scientific methods. With each advance of science a wider range of phenomena is woven into a harmonious pattern with those previously integrated, and there is no evidence that this process is approaching finality. It is indeed being markedly accelerated. Only if it were dwindling, only if there were plain indications that certain aspects of the world could never be embraced by science, would the divorce of scientists from politics perhaps become defensible.

Admittedly the time is distant when psychology will become an exact science, when ethics will become a science even of the most rudimentary kind, and when domestic and international politics will be inspired by the scientific method. But everything goes to show that, however remote, such an end is not inherently unattainable; and to recoil from a task because of present difficulties and future uncertainties is unworthy of the spirit and service of science.

There are, however, more immediate directions in which we believe the political duty of the scientist to be plain. In all parts of the world men and women with scientific training are profoundly dissatisfied with much of the prevailing government machinery. They see complete ignorance of, or indifference to, those principles of knowledge which, in the last two hundred years, have yielded more 'demonstrable truth' than was accumulated during the whole of man's previous existence. They see wilful distortion of fact, opinion swayed by prejudice or emotion, weighty decisions made upon the basis of scanty and often erroneous information. They see little of calm suspension of judgment, of inflexible purpose to advance step by step from the known to the unknown, of repression of the personal factor in reaching conclusions from ascertained data. Surely here are very real and very urgent reasons for action. If scientists withhold themselves from politics, in which they perceive so much that is wrong which they could help to improve, they are withholding the leaven that might leaven the whole lump.

Present conditions add insistence to another aspect of the matter. We see with sorrow and bitter resentment that the gifts of science are being perverted to cause death and destruction. The plea not seldom made by men of science that they are not responsible for the uses made of scientific knowledge cannot be accepted at its face value. It is sincere, but it evades a vital issue. Men who create knowledge that has immense power, either for good or for ill according to its use, have a special responsibility to exert themselves to the utmost in favour of the good and against the ill. In the circumstances in which

we live, this responsibility cannot be discharged if scientists are content merely to be in the world and not of it.

Scientists are not alone in wishing to be left in peace and quietness so that they may get on with their job. But it is within their capacity to make to the common weal a contribution unique both in present quality and in potentialities for the future. It is therefore clear that, before determining to stand outside the political life of his time, the scientist should searchingly inquire of himself whether such action is consistent with the discharge of his full duty to humanity.

The seventeenth-century background

The Royal Society of London, whose early history is recounted in this number of ENDEAVOUR by Colonel Sir Henry Lyons, holds undisputed pride of place among all scientific societies in the world. It was founded in the seventeenth century, a period which covers achievement of the first magnitude in physical science. The beginning of the century saw the first-fruits of the reaction against the decadent Scholastic philosophy; its later years witnessed the publication of the *Principia*—a work which raised the great structure of classical dynamics in a form which still serves, with few exceptions, for the interpretation of large-scale phenomena.

The part played by collective work in the development of seventeenth-century physical science is a major one. Francis Bacon had seen visions, and in his House of Solomon, the end of whose foundation 'is the knowledge of Causes and secret motions of things,' he describes a Utopian research institution with an imposing apparatus of underground workrooms, mountain observatories, biological stations, chemical, physical and physiological laboratories, all in charge of teams of workers—the Pioneers 'who try new experiments such as themselves think good,' the Depredators, 'who collect the experiments which are in all books,' the Merchants of Light, the Mystery-men, the Compilers, the Benefactors, the Lamps, the Inoculators, and the Interpreters of Nature, whose functions we leave the curious reader to deduce from their names.

In some of its ideals a forerunner of Solomon's House, the Accademia dei Lincei, at Rome, had a chequered existence which extended over the first half of the seventeenth century. Its successor, the Accademia del Cimento (1657–67), at Florence, set up a high standard of experimental philosophy. All the efforts of the academicians were directed towards 'experimentation, the creation of instruments, the establishment of standards of measurement and of exact methods of research.'

The Accademia del Cimento is long dead. Our own Royal Society, but a few years younger than the Accademia, still flourishes. It is strange that the Society should have its first beginnings, as Sir Henry Lyons tells us on another page, in a period the most disturbed in all British history. The quarrel between King and Parliament came to a head in 1642 and England was plunged into civil war. After swaying doubtfully for some years, the war ended in the defeat of King Charles I, and his execution in January 1649 ushered in an eleven years' republican interregnum and the Protectorate (we should now call it the dictatorship!) of Cromwell.

Yet through all these brawling years of civil strife and dissension, men 'inquisitive into natural philosophy and other parts of human learning, and particularly of what has been called *New Philosophy* or *Experimental Philosophy*,' met in London and in Oxford, and, in a planless way characteristically British, sowed the seed which developed into the Royal Society of London. Is there some influence in a period of war when, although (again to quote the words of Wallis) 'academical studies were much interrupted in both our Universities,' men's minds are quickened and turned to the increase of knowledge? It may be a coincidence, yet it has happened so frequently as to indicate a causal connection. If it be so, we see again that there is something of good even in the evil of war.

Contributions and correspondence should be sent to the editor: E. J. Holmyard, M.A., M.Sc., D.Litt., F.I.C., Imperial Chemical Industries, Nobel House, Buckingham Gate, London, S.W.1. Scientists engaged in research of an interesting or important character are invited to send short notes on work in progress and results obtained.

The evolution of the solar system

SIR JAMES JEANS

The span of human life is so short that the solar system, except for its more obvious periodic phenomena, seems to us to be the same yesterday, today, and for ever. Yet it is subject to the universal flux, and its present form is the result of countless ages of change. Sir James Jeans offers an absorbing theory of its probable evolution.

The time is the year 1751, the scene a nobleman's house in East Prussia. The 27-year-old tutor to the family is reading a Hamburg periodical and happens upon a review of a book, *Original Theory or New Hypothesis of the Universe, founded upon the Laws of Nature, and solving by mathematical Principles the General Phenomena of the Visible Creation*, by Thomas Wright of Durham, England.

The tutor's name is Immanuel Kant. He feels a special interest in the British, claiming to be of British descent; his paternal grandfather, he says, was a Scot who spelt his name Cant. This may have resulted in his paying more attention than he would otherwise have done to the book of the unknown Englishman. In time he will be famous as the greatest of German philosophers, but at present he is more interested in science than in philosophy. He has, we may imagine, heard of various attempts to explain the visible creation in mathematical terms.

There was, for instance, Plato's belief that the distances of sun, moon and planets stood to one another in the ratio of the numbers 1, 2, 3, 4, 8, 9, 27—the so-called 'proportions of the double intervals.' There was the further idea, also attributed to Plato, that the planets had all been created at some enormously remote point in space—at infinity, as we should now say—and allowed to fall sunward from the hand of their Maker, the speed which each planet gained in its fall being changed into an equal transverse speed as soon as the planet reached its appointed orbit. This hypothesis accounted very simply for the rapid motions of the inner planets, but—as Newton showed in a later age—not for the speeds actually observed; the hypothesis could only be reconciled with these by supposing that the sun's gravitational force on each planet had been doubled at the moment when the direction of its motion was changed.

Again, there was Kepler's view of two thousand years later that the distances of the planets from the sun were determined by the geometry of the regular solids. He thought that if spheres were drawn round the sun, having the same diameters as the planetary orbits, then these spheres would be of such sizes that one of the regular solids—the cube, the tetrahedron, the octahedron, the dodecahedron and the icosahedron—would fit exactly between each pair of adjacent spheres, the inner sphere being inscribed in it and the outer sphere circumscribed around it. After thinking very highly of this theory for a time, Kepler abandoned it in favour of the view that the planetary distances stood to one another in ratios which were the frequency-ratios of harmonious musical intervals.

Kant saw at once that Wright's work contained something very different from this kind of mathematical fantasy. The English writer did not treat the astronomical universe as something for ever fixed, as Plato and Kepler had done, but as something for ever changing under the blows rained upon it by the passage of time.

Kant seems to have made no attempt to procure a copy of Wright's book—the state of his finances may have precluded that—but set to work to think out for himself in what way the operation of natural laws might have moulded the astronomical universe into its present form. In this way evolutionary concepts were introduced into the astronomy of the solar system—mainly by Kant, although Wright must at least be given the credit of having pulled a trigger very effectively.

Four years later Kant published the result of his meditations under the title *Allgemeine Naturgeschichte und Theorie des Himmels*, with the generous acknowledgment to Wright: 'It was reserved for an Englishman, Mr Wright of Durham, to make a happy step with a remark which does not

seem to have been used by himself for any very important purpose, and the useful application of which he has not sufficiently observed.' And, not having read Wright's book, he had further to confess that he could neither 'define the exact boundaries' between his own views and those of Wright nor point out in what ways he had advanced beyond Wright's sketch, and in what ways he had merely imitated it.

Kant pictured the primaeval universe as a chaotic cloud of specks of dust and particles of matter of various kinds, some standing at rest and others darting hither and thither in purely random motion. Such was the raw material out of which the operation of natural laws was to be made to fashion a universe of sun, moon and planets. Newton had contemplated much the same state of things some sixty years earlier, but there is no reason to suppose that Kant was acquainted with his conclusions. In a letter to Bentley, Newton had written:

> It seems to me that if the matter of our sun and planets, and all the matter of the universe were evenly scattered throughout all the heavens, and every particle had an innate gravity towards all the rest, and the whole space throughout which this matter was scattered was finite, the matter on the outside of this space would by its gravity tend towards all the matter on the inside, and by consequence fall down into the middle of the whole space, and there compose one great spherical mass. But if the matter were evenly disposed throughout an infinite space, it could never convene into one mass; but some of it would convene into one mass, and some into another, so as to make an infinite number of great masses, scattered great distances from one another throughout all that infinite space. And thus might the sun and stars be formed.

We shall return to the second of these alternatives later, for it describes the way in which we still imagine the sun and stars to have been formed, but it was with the former that Kant was concerned, and he reached much the same conclusions as Newton. He pictured order as gradually emerging out of chaos; the primaeval matter begins to cohere under the mutual gravitation of its parts, until finally the centre of the picture is occupied by a vast continuum which will ultimately figure as the sun. Kant thought that the mere act of cohering into a single body would set this mass into rotation, but here he went wrong. He further supposed that it would gradually cool, that as it cooled it would contract, that as it contracted it would rotate ever faster and faster, until finally its rotation became so rapid that it could no longer hold together as a single body, but burst like an overdriven flywheel. Masses of gas were thrown off from its equator and these, cooling still further, became first liquid and then solid. These solidified masses, according to Kant, formed the planets, of which our earth was one.

Forty years later the great French mathematician Laplace independently advanced what was practically the same hypothesis—independently, because he says that, so far as he knows, no one except Buffon had thought about these problems before himself. Laplace was an incomparably better mathematician than Kant, so that his theory is not only stated in better form than Kant's, but is also mathematically more accurate. Laplace knew better than to suppose that the mere act of condensing could set a nebular mass into rotation—for rotation cannot be generated out of nothing—and so postulated a nebulous mass which was in a state of rotation from the outset. This was in no way objectionable, for we know now—although Laplace did not—that most of the nebulae in the sky are in a state of rotation. Cooling, contraction, and speedier rotation now supervene, just as in Kant's theory. Laplace's keen mathematical insight next enabled him to trace out the further changes which were to be expected. He saw that, with increasing rotation, the mass would flatten out, its equatorial regions protruding more and more, until finally a ring of matter would detach itself from the equator and be left revolving round the ever-shrinking sun—and here Laplace pointed to Saturn (figure 1) with its encircling rings of matter as evidence that the state of things he was imagining could really occur in nature. Such an ejected ring of matter, he thought, would ultimately condense into a planet.

While this was going on, the central sun would still be cooling, contracting, and increasing its speed of rotation, so that after a time the same sequence of events would repeat itself and result in the birth of a second planet, to be followed in due course by a third, a fourth, and so on. Even this is not the end of the story, for the planets would themselves cool, contract and increase their speeds of rotation, just as their parent sun had done before them. Finally, by a small-scale repetition of the processes already described, families of satellites would be born to circle round the planets.

Laplace did not support his conjectures with detailed mathematical calculations, but a later

FIGURE 1 – *Saturn with its encircling rings. Laplace thought that the sun had thrown off similar rings which had ultimately condensed to form the present planets.*

FIGURE 2 – *The Great Nebula in Andromeda. This is one of the nearest of the extra-galactic nebulae. It is of about the same size and mass as our own system of stars bounded by the Milky Way, and contains enough matter to form about* 100,000 *million stars like our sun. Most of this appears to have already condensed into stars, and many of the types of stars—novae, variable stars, giant stars, and super-giants—which are familiar to us in our own system have been recognized in it.*

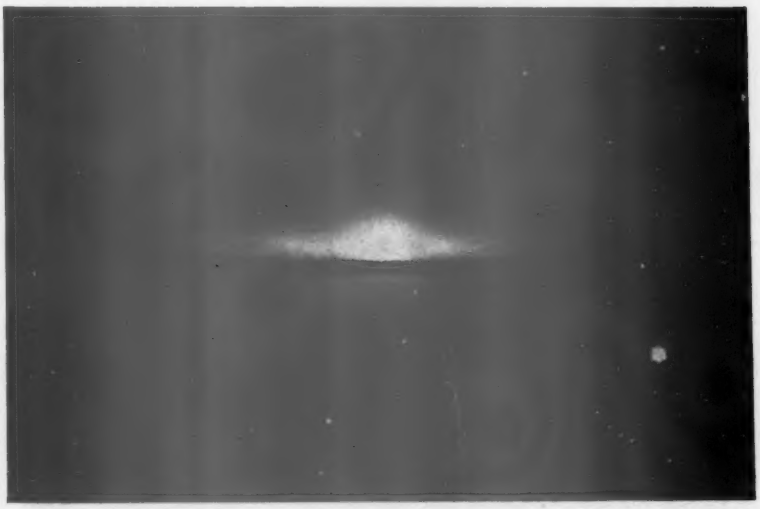


FIGURE 3 – *The extra-galactic nebula N.G.C.* 4,594 *in Virgo. Here we see the central flattened mass surrounded by a layer of cooler—or at least darker—matter in the equatorial plane of the nebula.*

FIGURE 4 – *The extra-galactic nebula N.G.C. 7,267. This is probably similar physically to the nebulae shown in figures 2 and 3, but is viewed from a direction perpendicular to its equatorial plane. The matter in its equatorial plane has already formed condensations which will produce, or more probably have already produced, clusters of stars.*

FIGURE 5 – *A huge solar prominence photographed by Evershed. The planetesimal theory imagines that in some past epoch far larger prominences were ejected by the sun and condensed into small solid bodies, by the aggregation of which the present planets were formed.*

generation of mathematicians has supplied these, and finds that all the processes imagined by Laplace are fairly true to life, provided that two conditions are fulfilled: the primaeval nebula must have been on a sufficiently ample scale, and must have been endowed with sufficient rotation to begin with. The first condition is fulfilled in the great extra-galactic nebulae (figures 2, 3, and 4), and the observed shapes of these are probably concrete illustrations of the sequence of configuration imagined by Laplace. But it is not fulfilled in an ordinary star. This is not on a large enough scale for Laplace's factors to operate, and when it breaks up from excess of rotation, it does not give rise to a family of planets; it more probably breaks into a 'binary system' consisting of two members, each of stellar size. The second condition—that of adequate rotation—is even more important since, as Babinet pointed out in 1853, it is quite impossible that the sun can ever have complied with it.

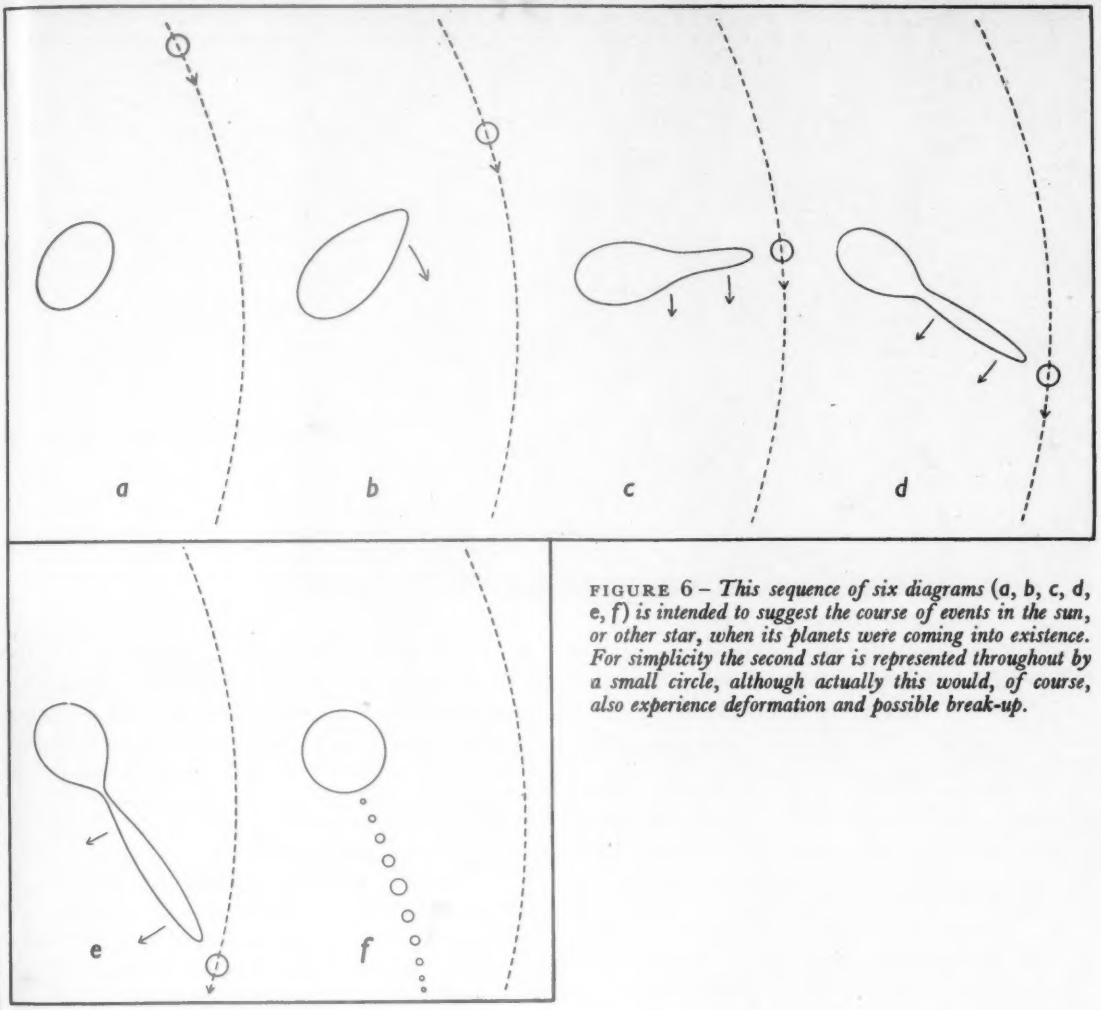
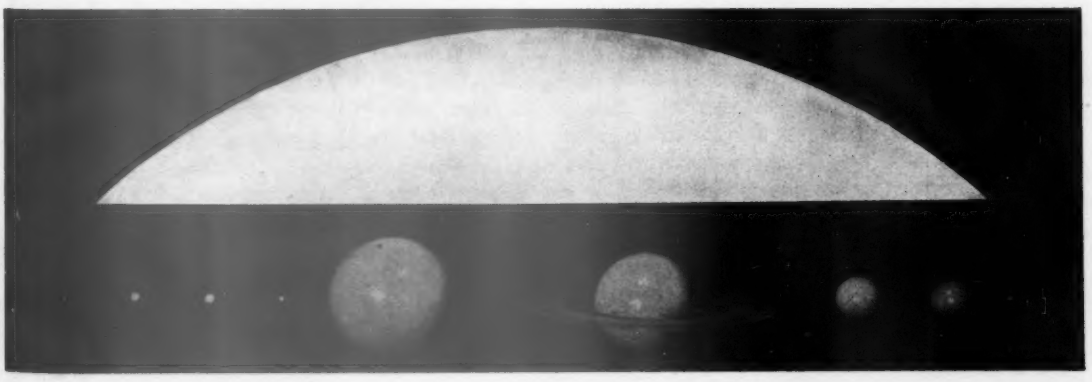


FIGURE 6 – *This sequence of six diagrams (a, b, c, d, e, f) is intended to suggest the course of events in the sun, or other star, when its planets were coming into existence. For simplicity the second star is represented throughout by a small circle, although actually this would, of course, also experience deformation and possible break-up.*

FIGURE 7 – *The planets drawn to scale as regards size, but not distance. The upper body represents part of the sun on the same scale. If the sun is supposed placed at the extreme left, the planets then follow in their proper order: Mercury, Venus, Earth (with moon), Mars, Jupiter, Saturn, Uranus, Neptune, Pluto.*



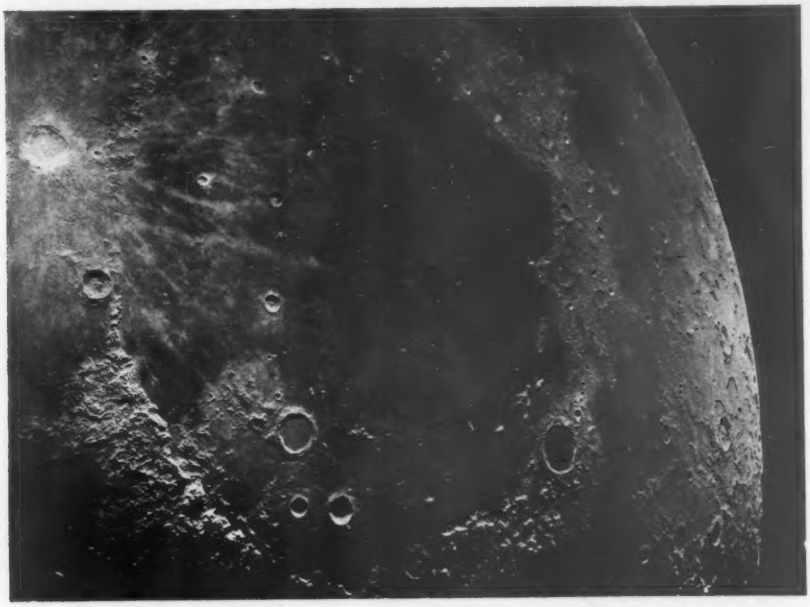


FIGURE 8 – *Part of the moon, showing the final product of the condensation of a mass of heated gas. The craters and vent-holes which cover the surface provide evidence of past eruptions of heated matter from the interior.*

For, throughout the whole sequence of changes imagined by Laplace, the system as a whole could never change the total amount of its angular momentum: whatever amount it had to start with, that it would retain to the present day. Because of this, it is easy to determine precisely what amount of angular momentum resided in the primaeval nebula—it was simply the total amount that we find today in the sun, planets, and satellites of the whole solar system.

Now this amount could not have effected even the first of Laplace's steps towards break-up. If we reconstruct the primaeval nebula by rolling all the members of the solar system into a single mass, which we then endow with this amount of rotation, we shall find no overdriven flywheel, but a mass rotating with such majestic slowness that its form is almost perfectly spherical; it will show even less flattening of figure than Jupiter does now.

Recent investigations have fully confirmed this objection, and have shown that it applies not only to Laplace's conjectures but also to every other conjecture which attributes the genesis of the planets to one star alone; it has now become abundantly clear that two bodies must have been involved—the planets must have had a father as well as a mother.

This is a conjecture which has often been made on its own merits. In 1750 Buffon imagined that the sun's family might have resulted from a collision between the sun and a passing comet, bits of the sun being splashed off and condensing to form the present planets. In 1860 Bickerton of New Zealand propounded a theory which was similar, except that Buffon's comet was replaced by a star and his head-on crash by a grazing collision: bits of the sun were scraped off by a passing star, and set spinning in the process. In recent years Jeffreys of Cambridge has returned to the same train of thought.

Yet a serious difficulty confronts all theories of this type. The two giant planets Jupiter and Saturn both possess extensive systems of satellites, and each planet with its satellites forms an almost exact replica, in miniature, of the main system of sun and planets. The systems of sun, Jupiter, and Saturn resemble one another so closely that it seems fantastic to attribute different origins to them. Yet we can hardly suppose that all three systems have been formed by grazing collisions; one collision which only grazes is a highly improbable event, and any theory which postulates three is simply asking too much.

In 1898, a Cambridge mathematician, W. F. Sedgwick, considered the possibility that the second body might produce planets without colliding with the sun at all, through tidal disturbances created in the sun. I put forward the same idea in 1901. In 1904 Chamberlin and Moulton, of the University of Chicago, suggested the same possibility independently and in far greater detail than I had done.

They imagined that in some remote past age

the sun had been liable to eruptions of the kind which now show themselves as solar prominences (figure 5), but of far greater intensity. While the sun was in this state, a wandering star happened to pass quite close to it. Its tidal action intensified the solar eruptions, puffs of gas being ejected with such speed that they broke clear altogether of the sun's atmosphere, and ultimately condensed into solid bodies out in space. To these the name 'planetesimal' was assigned, since they were of far less than planetary size; in time large numbers of them fell together, and by their aggregation the present planets were formed. But this theory suffers from the defect that puffs of hot gas of planetesimal size would not condense into solid bodies; their internal gas-pressure would cause them to expand, and they would merely scatter into space like the gas escaping from a leaky gas-burner.

In 1916 I tried to trace out the chain of events which would occur when a second star approached fairly close to the sun, and found that the tidal action of the second star might suffice of itself to break up the sun and produce a family of planets without calling in any intermediate mechanism such as eruptions of planetesimals. The process is briefly as follows:

Just as the proximity of the moon raises tides in our atmosphere, on our oceans, and even in the solid body of the earth, so the close approach of a second star would raise tides in the gaseous sun. But these tides would be very different from the minute tides which the moon raises in the earth. These are minute, partly because the moon is much smaller than the earth, and partly because it is at the relatively great distance of about sixty of the earth's radii away from the earth—and tidal effects diminish as the cube of the distance. But a second star, at least equal to the sun in mass, approaching to within two or three radii of the sun, would produce tidal effects which would be immense. Calculation shows that, in place of the mild elevation of surface we find in terrestrial tides, we should see a huge mountain of gas rear itself up on the sun as the other star approached, its rate of growth increasing almost without limit, until it ultimately darted out as a long tongue of gas—pulled out of the sun by the gravitational attraction of the second star and pointing towards the second star (figure 6 (*a*-*d*)).

Now any column of gas, provided only it is on a sufficiently large scale, is unstable through the operation of the process we describe as 'gravitational instability.' When a musician plays a flute, he sets the air inside it into vibration, slight condensations of air occurring at certain points with compensating rarefactions at others. As soon as he leaves off blowing, these inequalities of density disappear; the excess of pressure at each point of condensation causes the gas at this point to expand and all unevennesses are soon smoothed out. But if the flautist and his flute are on the astronomical scale, unevennesses are not smoothed out in this way; on the contrary, they continually increase. The reason is that, as the scale of size increases, a new physical factor becomes increasingly important, namely the mutual gravitation of the particles of the gas; this causes the gas to concentrate still more strongly round any points at which the density of gas happens to be above the average, no matter how slight the excess of density may be. Thus when irregularities of density once occur in a column of gas of astronomical size, they continue to increase until most or all of the gas is collected round the various nuclei of condensation. This is a small-scale version of the second of the two processes described by Newton in the letter already quoted, and to it the tidal theory ascribes the birth of the planets. We can calculate what would be the masses of the condensations formed under any specified physical conditions, and find that those formed out of the tidally deformed sun would have just about the masses of the actual planets.

Before these nuclei of condensation began to form in the column of solar gas, this would be shaped like a cigar or torpedo, one of the two pointed ends having formed the peak of the tidal mountain, and the other consisting of the last thin dribblings of matter which left the sun just as the tidal pull of the receding star was ceasing to be effective (figure 6 (**e**)). And when the condensations formed, we should expect to find the most massive occurring near the middle of the column, where matter had been most plentiful, and the least massive at the ends where matter had been most sparse. If the planets really are the remains of such condensations, we ought to find the most massive planets occupying positions near the middle of the sequence, with the smaller planets near to, and far away from, the sun. This is precisely what we do find. Actually Jupiter, the largest and most massive of all the planets, occupies the middle position in the sequence, and as we recede from this in either direction the planets fall off, both in size and mass, until we come to Mercury at the one extreme and Pluto at the other (figure 7).

These planets now move in almost circular orbits, but we know that this results from their having ploughed their ways for thousands of millions of years through the debris of gas and dust left by the cataclysm to which they owe their being. But when they first started their existences as detached masses, we may suppose that their orbits were of all kinds and shapes, possibly showing very little regularity beyond that of all lying in one plane—the plane in which the star had moved which had brought them into being. At times a planet would pass so near to the sun as to be broken by its tidal pull, just as its parent sun had been broken before it. Even the giant mass of Jupiter might suffice to break up a planet which wandered too near to it. In such ways as this we can imagine the systems of satellites being formed to circle round the planets, as well as the cloud of asteroids which occupies the planetary gap between Mars and Jupiter.

We may picture the new-born planets as cooling down, liquefying in whole or in part, and finally solidifying. Some of them might give birth to satellites while they were still in the gaseous state, others only after they had liquefied or even solidified. On the whole, the largest planets would be most likely to generate their satellites while they were still gaseous, for they would cool most slowly and so stay longest in the gaseous state. Thus the process by which they give birth to satellites is like that by which they were themselves born out of the sun, and we should expect them to produce systems which would be small-scale replicas of the solar system itself. But it is different with a planet which is already liquid or solid; mathematical investigation shows that the satellites produced by this will be fewer in number but larger in comparison with their parent planet. The gaseous planet produces many but small satellites, while the liquid planet produces few but large.

This accounts exactly for what we find in the solar system. The two largest planets, Jupiter and Saturn, cooling very slowly, would almost certainly still be gaseous when their satellite systems were born, and these, as we have seen, are almost exact replicas of the system of the sun. But of the smaller planets, some have no satellites at all, while others have satellites which are few in number but relatively large in size, and this is just the type of family to be expected for planets which were already largely liquid when their satellites were born.

The transition from one type of satellite system to the other occurs with Mars in the one direction and Uranus in the other. This suggests that Mars and Uranus were the smallest planets to remain gaseous for any length of time. If so, we can explain another feature of the solar system, namely the abnormal smallness of these two planets, both in size and mass, for their positions in the sequence of planets. For if these planets remained gaseous for a considerable time, they, with their feeble gravitational pulls, would obviously suffer more than the larger planets through the dissipation of their outer layers into space. Thus they may well be relics of far larger masses, and if we could restore their dissipated atmospheres, we might find the whole sequence of planets decreasing regularly in both size and mass as we receded from the central giant Jupiter.

Such is the tidal theory of the evolution of the solar system. It is based on *verae causae;* tides really occur in nature, so that some planetary systems must have been born in the way it describes. To the question as to whether our own planetary system was born in this particular way, the only answer is that the hypothesis that it was so born seems to account for a large number of the observed features of the system—too many, it may be argued, for the agreement to be purely accidental.

But the tidal theory occupies only one corner of a canvas on which we attempt to portray the evolution of the universe as a whole.

Let us imagine with Newton and Kant that the universe has evolved out of chaos, and let us try to reconstruct the primaeval chaos by imagining all the matter of the universe to be uniformly scattered throughout space. At present the matter is far from being uniformly scattered, most of it being aggregated in the detached units we describe as 'galaxies.' These comprise our own system of stars, bounded by the Milky Way, and a vast number of similar structures which we describe as 'extra-galactic' nebulae (figures 2–4). These nebulae are vast aggregations of stars, each containing about as much matter as 100,000 million stars like our sun. We speak of the amount of matter they contain rather than of the number of stars, because our knowledge is obtained by weighing the whole galaxy—by calculating its gravitational pull—and not by counting its stars, which would present an impossible task.

The average distance between galaxies is perhaps a million light-years, which means that

the average amount of matter per cubic metre of space is about 200 atoms of hydrogen or 1 atom of mercury.

If we could distribute all the matter of the present universe evenly throughout the whole of the present space, this is the density of matter we should obtain. But we must not assume that the primaeval chaos of perhaps 3,000 millions of years ago was of this same density. For it seems probable that space is expanding, and this at such a rate that its volume must have been increased a hundred-fold by 3,000 million years of expansion. If we assume that primaeval matter occupied only a hundredth part of its present space, and so had about a hundred times its present average density, and also that it consisted mainly of free electrons at a temperature not very far from that of melting ice, we find that gravitational instability would operate and divide the matter up into condensations, each of which would contain about as much matter as an average galaxy. Here, then, we have a very simple interpretation of the existence of these detached galaxies. We cannot, of course, be sure that they came into existence in the way we have described, but as before it seems unlikely that the agreement between theory and observation can be merely accidental.

We have already noticed that these galaxies have precisely the shapes which would be assumed by masses of gas in rotation, and in many of them rotation can actually be observed by spectroscopic methods. Thus it seems likely that the majority are in a state of rotation and, although we do not know what produced this in the first instance, it is clear that currents in the original matter might result in a slow rotation which would become intensified as the nebulae contracted. This would lead to the consequences that Laplace predicted, so that it is not surprising that the shapes of the nebulae fit on to the sequence described by Laplace.

At one point of this sequence a thin layer of matter has been thrown off, and lies, slowly rotating, in the equatorial plane of the nebula (figure 3). Here again gravitational instability must come into operation; the layer of gas would break up into condensations, and calculation shows that these would be of stellar mass. In this way, then, we can suppose the stars to have been formed: the nebulae were their birthplaces.

This brings us back to that corner of the picture which we have already described. The new-born stars contract, and ultimately form stars of the kind we know, a pair of them occasionally passing so close to one another that their tidal interaction produces a planetary system. The process of contraction must be exceedingly rapid; for a long time it was thought that its rapidity must be such that there could be no appreciable chance of planets being produced while it was in progress. But this overlooked that the immense size and tenuous texture of the new-born stars would make them especially susceptible to tidal action. Whatever the origin of our particular system may have been, it is reasonably certain that a large number of planetary systems have been formed through tidal action, and that a very large proportion of these were formed while their suns were still in a semi-nebulous state; this is a matter of mere statistical calculation. Our own system seems to carry intrinsic evidence that it was born in the same way as this majority—i.e. while the sun was still in a semi-nebulous state. For no other hypothesis can explain the immense extent of the system; if the planets had appeared when the sun had shrunk to about its present size, no available force could have taken matter out of the sun and set it revolving in the orbit of distant Pluto or could have made the planets rotate as rapidly as they now do.

When the planets were thought to have been produced by masses of the sizes of the present stars, it was clear that effective tidal encounters would be exceedingly rare events, whence it was concluded that planetary systems must be rare objects in the sky. It now appears that an additional and far greater number of effective tidal encounters must have taken place while the stars were large, so that planetary systems must be far commoner objects than was previously thought. What has happened to our sun is not the normal fate of a star, but it is much nearer to normal than we used to think, and it is a fate which must have befallen a fair proportion of the stars. And as the total number of stars is so great—being at the lowest computation greater than the number of blades of grass on the surface of the earth—it follows that the total number of planetary systems must be almost inconceivably great. Out of the vast total number, there must be millions which are almost exact replicas of our solar system, and millions of their planets must be almost exact replicas of our earth. Has life developed on these as it has here, and, if so, has it followed the same uneasy paths as our terrestrial life? We can only guess, for science has nothing to say on the question.

The Royal Society of London

Part I

COLONEL SIR HENRY LYONS

The Royal Society of London is by far the most celebrated scientific society in the world, and election to its Fellowship or Honorary Fellowship is a justly coveted distinction. Colonel Sir Henry Lyons has been engaged for many years in an exhaustive study of the history of the Society, of which he was formerly Foreign Secretary and later Treasurer. In the present article—the first of two—Sir Henry describes the birth of the Society and its early development.

On 15th July, 1942, the Royal Society had been in existence for 280 years since its first Charter was granted by King Charles II. The date of this Charter has been taken ever since as that of the Society's foundation, although the Philosophers' Society, which was formally constituted on 28th November, 1660, and six months later was named by the King's permission 'The Royal Society,' had been active for about 18 months when the Society received its incorporation by the Charter.

The influences which led to the foundation of the Society can be traced back in Britain for more than a century earlier, as the new or experimental philosophy spread rapidly among those who were studying natural science. The changes in intellectual outlook which had taken place in southern and western Europe during the fifteenth and sixteenth centuries exercised a notable influence on thought and learning in the seventeenth. Such men as Leonardo da Vinci, Copernicus, Galileo, and others had realized that greater certainty in the study of natural philosophy was attainable by research aided by experiments than by accepting what had been transmitted in the writings of earlier authorities. The new teaching spread widely, and in England William Gilbert of Colchester, physician to Queen Elizabeth and James I, investigated the attraction between magnets by means of a series of experiments which led him to the conclusion that the earth itself must be a great magnet; while William Harvey, who studied at Cambridge and afterwards at Padua, followed the new method of observation and experiment in the investigations which led him to the discovery of the circulation of the blood. About the same time Francis Bacon, Lord Verulam, was putting forward his views on how the advancement of knowledge might be utilized as so to bring about an improvement in man's conditions. His writings were widely read at Cambridge, where a number of learned men were taking an active interest in the progress of the new philosophy. About 1645, several of the younger men who had come under this teaching and influence began to meet in London to discuss the many problems which

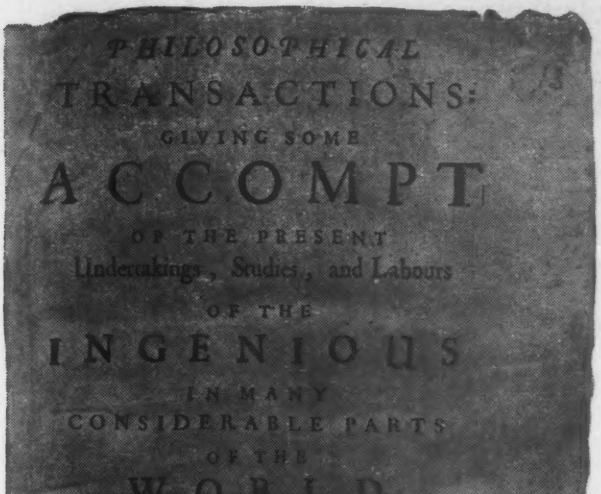
The Hon. Robert Boyle.

awaited solution, or at least serious study. One of the most eminent of them was Dr John Wallis, a divine and mathematician of distinction who graduated at Emmanuel College, Cambridge, and was ordained in 1640. He was at this time in London, since studies at the universities were much interrupted by the Civil War between King and Parliament; but he and about a dozen of his friends arranged to meet weekly if possible to discuss philosophical matters and to make experiments at Gresham College. During the next three or four years others joined them, to form what was known as the Philosophical or Invisible College; these included Dr J. Wilkins, who was Chaplain to the Elector Palatine; Dr J. Goddard, a physician and chemist; Dr C. Ent, a friend and supporter of William Harvey; the Hon. Robert Boyle, who had just returned from France; Dr W. Petty, a surgeon; and some others.

Dr J. Wilkins.

About 1648 several of their small company were transferred to Oxford. Wilkins was appointed Warden of Wadham College by the Parliamentary Party; Goddard was made Warden of Merton College, after being Physician-in-Chief to Cromwell in Ireland and Scotland; Wallis was appointed Savilian Professor of Geometry; and another Cambridge philosopher and mathematician, Dr Seth Ward, was appointed Savilian Professor of Astronomy. Robert Boyle, after his return from France in 1646, had retired to his house in the country to carry on his studies in chemistry; but in 1654, at the invitation of Wilkins, he took up his residence in Oxford, where he remained until 1668, when he moved to London. Thus it came about that from 1649 a group of the philosophers carried on their studies at Oxford for some years but continued to meet their colleagues at Gresham College whenever they had occasion to visit London.

After the restoration of the monarchy, the philosophers at Oxford and in London resumed their meetings at Gresham College, and on 28th November, 1660, a dozen of the most zealous of them agreed that they should form a scientific society to promote the new philosophy in this country. Sir Robert Moray, who had served both Charles I and Charles II and had been trusted by both, was chosen as being the most suitable of their number to inform King Charles II of their decision and of the aims of the Society. A week later he brought word from the Court that 'the King did well approve of this designe, and would be ready to give encouragement to it.' Thus the Philosophers' Society was founded, and about six months later the King approved a suggestion made to him by John Evelyn that the new Society should be known as the Royal Society. In the autumn of 1661 the philosophers submitted a petition to the King for a Charter of Incorporation, and this was granted on 15th July, 1662, thus providing them with a safeguard against the Society's suppression by opponents who soon became active. There were, however, some additional matters which the Fellows desired to have included in their Charter; they therefore prayed the King for a new Charter which should embody these additional provisions. This, the second Charter, was presented on 22nd April, 1663, and is the Charter by which the Society has always been, and still is, governed. In these Charters the administration of the Society and the direction of its policy are entrusted to a Council consisting of twenty-one Fellows of the Society, including the President and officers, who are elected annually at the anniversary meeting


PHILOSOPHICAL
TRANSACTIONS:
GIVING SOME
ACCOMPT
OF THE PRESENT
Undertakings, Studies, and Labours
OF THE
INGENIOUS
IN MANY
CONSIDERABLE PARTS
OF THE
WORLD
Vol. I.
For Anno 1665, and 1666.
In the SAVOY,
Printed by T. N. for John Martyn at the Bell, a little without Temple-Bar, and James Allestry in Duck-Lane, Printers to the Royal Society.
Presented by the Author May 30th 1667.

The title-page of the first volume of the Philosophical Transactions.

of the Fellows, ten of the councillors being replaced each year.

During the first ten years the Society prospered; the Fellowship rose to 215; a scientific periodical, the *Philosophical Transactions*, was first published in March 1665, and was at once recognized as the leading journal of its kind; meetings at which communications were received and discussed were held weekly. Foreign men of science occasionally attended these meetings and invariably expressed their admiration of the order and discipline which prevailed.

After the first few years of the Society's activity had passed, interest in it temporarily declined; membership fell off, and it was difficult to meet the expenses of publishing the *Philosophical Transactions*. When the Society accepted Sir Isaac Newton's *Principia* in 1686 and ordered its publication, there were no funds available to meet the cost until Edmond Halley undertook to provide the necessary sum. Indeed, from 1675 to the end of the century was a period of exceptional difficulty. Had it not been for the zeal and energy of Sir John Hoskins, a vice-president, and of Dr Hans Sloane, the senior secretary, the Society might not have weathered the storm. Although the scientific Fellows were few in number during the seventeenth century, they included a large proportion of eminent men; among these may be mentioned the mathematicians Barrow, Wallis, Newton, Gregory; the astronomers Halley, Flamsteed; the physicist Hooke; the chemist Boyle; the physicians Glisson, Goddard, Mayow, Lower; and the naturalist Ray.

In 1696 Sir Isaac Newton had been appointed Warden of the Royal Mint on the recommendation of Lord Halifax, and this provided him with an official post in London which brought him into closer contact with the Society and its Fellows. His great prestige as a leader of science indicated that he should be the Society's president, but it was well known that he would not accept nomination so long as Robert Hooke lived, since the latter's jealous criticisms and contentious claims for priority of discovery when others put forward any claim were more harassing than Newton's sensitive nature could support. Early in 1703, however, Robert Hooke died and Lord Somers, the president, at once announced that he did not desire nomination at the next anniversary meeting; Newton was therefore elected unopposed.

Nothing could have been more fortunate for the Society, for the new president's eminent position in the world of science greatly increased the esteem in which the Society was held. Newton did not restrict his activity to the routine duties of his new office, but gave his close attention to the administration. He attended the meetings of the Council most regularly—missing only fourteen of the 175 Council meetings which were held during his tenure of the presidency—and attended the ordinary meetings of the Society as regularly. He also introduced the practice that the Council meetings should be attended by both the secretaries instead of by one only. In these and other ways he greatly improved the efficiency of the administration and laid down a procedure which was followed with but trifling alterations for more than a century.

The number of scientific Fellows in the Society between 1663 and 1700 rarely exceeded fifty, and their subscriptions were not enough to meet its

expenses. From the first, therefore, a number of candidates who had no scientific knowledge or interests were admitted, and these were usually twice as numerous as the scientists; they also were always in the majority on the Councils. This state of things did not tend to advance the scientific activity of the Society, but it was not until 1820 that scientific Fellows were in the majority on the Councils, and then, as we shall see later, the beneficial results of their influence were immediate and highly satisfactory.

In 1730 Newton's successor, Sir Hans Sloane, an administrator of much experience, instituted the practice of taking legal action against those Fellows who did not pay their subscriptions, and thus enabled the Council to put an end to the financial difficulties of the Society. From this time onwards the number of Fellows increased steadily, from 130 when Newton became president to 400 by 1775, and to 600 by the end of Sir Joseph Banks's presidency in 1820. The proportion of non-scientific to scientific Fellows, namely about two to one, varied little during the whole of the eighteenth century. Presidents like Lord Macclesfield and Lord Morton, both of whom were amateur astronomers of distinction, and Sir John Pringle, a well-known physician, notably advanced the scientific activity of the Society; but Martin Folkes and James West, who were antiquaries, did little in this direction. Towards the end of the century the secretaries were frequently historians and literary men, and in some years none of the officers was a man of science.

During the eighteenth century administration had not been conspicuously successful from the scientific point of view, but this came to an end with the presidency of Sir Joseph Banks, who guided the path of the Society for almost forty-two years. He was thirty-five years of age when he was elected president, the youngest of the Fellows who have held that distinguished post; he was a wealthy landowner, interested in botany, horticulture, agriculture, and the raising of stock; he had travelled extensively and had generously financed the collecting of plants, etc., from foreign countries with the object of acclimatizing them here or in British colonies. During most of his presidency, one of the secretaries, Dr Blagden, was a man of scientific training, but the treasurer and the other secretary were usually literary men; Banks himself, in spite of his general interest in agriculture and natural history, gave his attention mainly to the utilization of the plants and other specimens which his collectors and travellers brought or sent to him.

It is disappointing to find that he made no attempt to strengthen the Councils of the Society by the addition of men of science; on the contrary, the average number of them was often lower during his presidency than had previously been the case, varying from seven to ten out of the total of twenty-one. There is evidence that towards the end of his life he realized that the Society must take up a much stronger scientific position in the future than it had hitherto done, for he expressed the opinion that W. H. Wollaston, an eminent chemist, was the most suitable of the Fellows to succeed him (an opinion not without incidental interest, since it shows that Banks estimated Wollaston more highly than Sir Humphry Davy).

Sir Joseph Banks highly esteemed the honour which the Society had conferred upon him, and carried out his duties with the utmost regularity and thoroughness; he regarded the Society as a trust committed to his care and desired to preserve

Dr John Wallis.

Early air-pump

it as free as possible from any detrimental change or harmful competition. It was probably this which caused him to oppose the establishment of such specialized societies as the Geological Society and the Astronomical Society, which in each case was supported by a number of serious and distinguished workers convinced that the efficient study of their subject required greater facilities for discussion and publication than the Royal Society could provide. Subsequent experience has amply proved the correctness of their contention.

The very inadequate representation of science on the Councils of the Society in the eighteenth century must have greatly reduced the scientific influence which the Society might have exercised during those years; that such representation should hardly show any increase over a period of a century and a half is in itself extraordinary unless it can be ascribed to the deadening effect of a majority in the Councils which neither knew, nor cared to know, anything about natural science, and whose instinctive attitude was to obstruct any movement in favour of it. There were always a number of Fellows whose interests lay in advancing the knowledge of the branch of science which they had made their own; and these were the men who were devoting themselves to research as their own genius prompted them. Some of them served but seldom on the Councils, while others gave their time to this duty on many occasions—probably from a public-spirited desire to help the Society rather than from a preference for such work. The independent researches of the astronomers, mathematicians, chemists, physicists, physiologists, physicians, and others who made reputations for themselves by their scientific work at the same time enhanced that of the Royal Society. For its part, the Society took pride in publishing their discoveries in the *Philosophical Transactions* and in providing them with rooms where they could meet their colleagues. Among such Fellows the following may be mentioned:

Royal Society treasure chest

mathematicians: A. de Moivre, C. MacLaurin, J. Stirling; astronomers: J. Bradley, N. Maskelyne; physicists: S. Gray, H. Cavendish; chemists: J. Priestley, W. Wollaston; physicians: R. Abernethy, J. Mead, J. and W. Hunter, J. Lind, and J. Pringle. Many of these men made fundamental discoveries in their respective domains of science and medicine, and helped to establish the great scientific tradition to which the Royal Society owes its pre-eminent status.

Mating discrimination in plants

K. MATHER

The mechanism of pollination in flowers is a subject of perennial interest not merely to the professional botanist but to gardeners, horticulturists, and all lovers of plant life. Dr Mather shows that older interpretations were often too naïve, and that—for instance—many flowers apparently adapted to wind-pollination are in fact self-pollinated. The accompanying photographs show very clearly some of the ways in which plants effect discrimination in mating.

Pollination is the critical stage in the life of a flowering plant. If a pollen grain, after its release from the anther where it was produced, falls on to the stigma, or receptive surface, of the female part of a flower, it germinates to give a pollen tube. The tube then grows down the style until it reaches the ovary, where, provided that it has not been forestalled by other tubes, it passes into an unfertilized ovule. Each pollen grain carries a male generative, or sperm, nucleus which is discharged by way of the pollen tube into the ovule. Here the sperm nucleus fuses with the female egg nucleus lying in the embryo sac, to give a cell which develops first into an embryo, as found in the ripe seed, and later, if conditions are favourable, into a mature plant. The pollen also plays a part in the production of other seed structures, but this is ancillary to its main role in reproduction.

Thus pollination is like mating in animals—the essential precursor of fertilization and sexual reproduction. A seed-bearing plant which fails to be pollinated leaves no offspring. Similarly a pollen-producing plant leaves no progeny if its

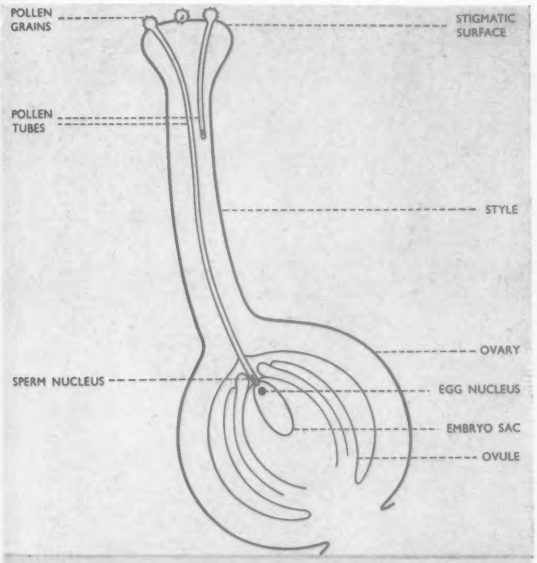


FIGURE 1 – *Pollination and fertilization. When a pollen grain falls on to the stigmatic surface it germinates to give a pollen tube, which grows down the style and penetrates into an unfertilized ovule. The sperm nucleus is carried down the tube and, when the latter reaches the ovule, passes into the embryo sac where it fuses with the egg nucleus to give a fertilized egg. Details of endosperm formation are omitted for simplicity.*

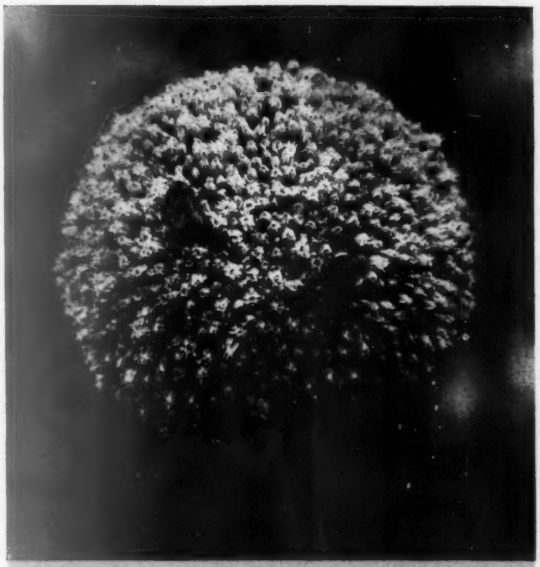


FIGURE 2 – *A flowering head of the leek* (Allium porrum). *Many flowers are massed together, so giving a striking head to which bees are attracted. Two bees can be seen working over the head. Some of the flowers are open and their anthers visible, but others are still closed. Anthers touch and deposit pollen on the under side of the bee, from which it is picked up later by stigmata. Pollen is thus transferred from flower to flower and plant to plant.*

FIGURE 3 (*above*) – *A plant of maize* (Zea mays). *Maize shows adaptation to wind pollination. The flowers are unisexual, those carrying the stamens being grouped into a 'tassel' at the top of the plant. Anthers may be seen hanging from the tassel. The female flowers are grouped into ears which are borne lower down the stem. The ears are protected by husk leaves, from which protrude the stigmata, one to each flower, in the form of long 'silks.' Two silks are visible in this plant. Pollen falls from the tassel and is blown about, some of it eventually falling on to silks. Pollen may, in this way, be transferred over considerable distances to other plants.*

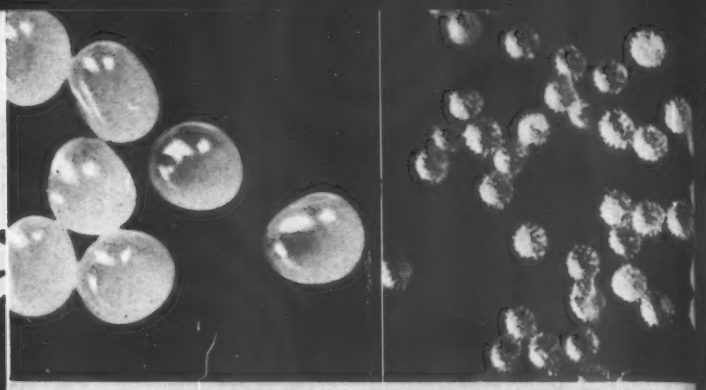


FIGURE 4 (*above*) – *Pollen grains of maize, on the left, and of the ga* (Dahlia variabilis) *on the right. Maize is adapted to wind pollination, its p smooth, dry and free running. Note how the smooth surface reflects high lights. the other hand, is insect-pollinated and its pollen is rough and sticky. The shows traces of the sticky anther juice, which is also responsible for the blurred appearance of the individual grains.* (× 150.)

FIGURE 5 (*right*) – *In-bred and cross-bred maize. The in-bred plants in the foreground are very small and weak in comparison with the cross-bred plants behind them. Maize is adapted to cross-pollination and, in consequence, in-breeding results in loss of vigour.*

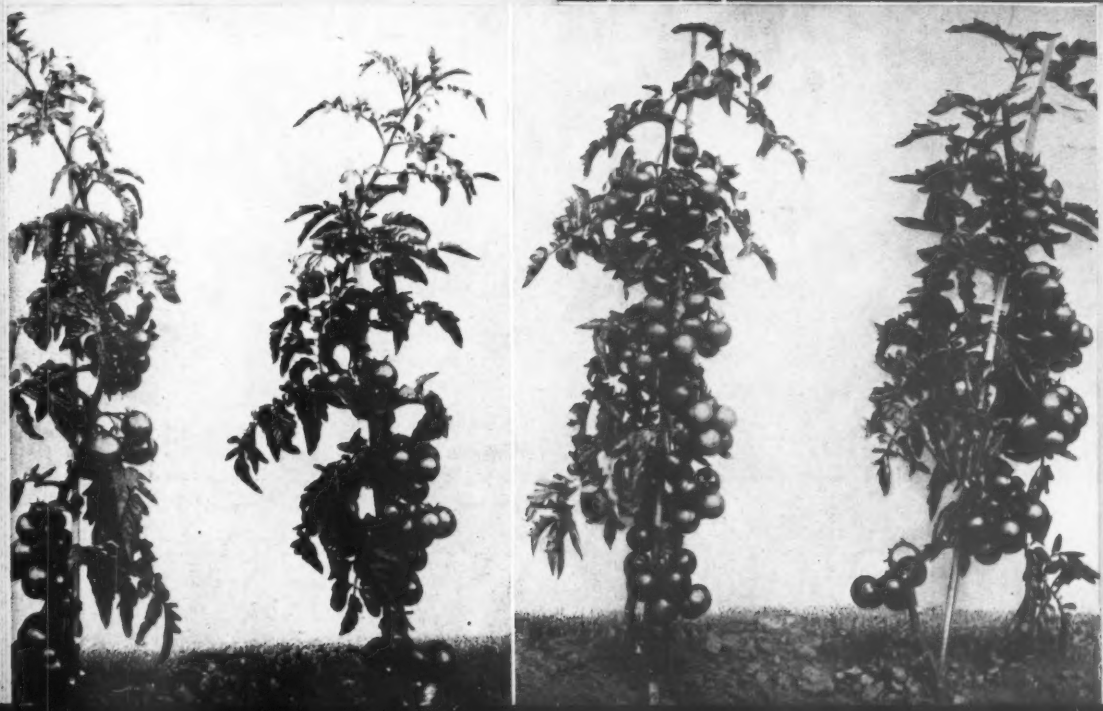


FIGURE 6 (*le and cross-bre* (Lycopersicu lentum). *Th plants on the more vigorous bred plants o The tomato sh self-pollinatio genetical st adapted to in- consequence, vigour follow in-breeding, a what happens

7 (*right*) – *A flowering shoot of the willow-herb* (Epilobium angustifolium). *rb is insect-pollinated, its flowers being coloured and striking. The anthers e the stigmata of the same flower mature. The chance of self-pollination is thus reduced. In the youngest flower (upper left), the anthers project forward and ursting to discharge their pollen. The style is directed downwards and the stigma aped and closed. The flower immediately below is older. Its anthers are to fall downwards, while the style has become more erect to occupy the position the anthers. The stigma is just opening and exposing its inner receptive surfaces. lder flowers on the right show the exposure of the stigmatic surfaces and withering ers to a more pronounced degree. The youngest flower could not receive pollen on but could dust pollen on to a bee. The older flowers could no longer place pollen but their stigmata could pick up such pollen as the bee is already carrying.*

FIGURE 8 (*left*) – *Fixed and stained preparation of pollen tubes growing in the style of* Oenothera organensis (*preparation and photograph by Dr D. Lewis*). *Pollination took place 45 minutes before the preparation was made. Some pollen grains (deeply stained at the top of the photograph) have germinated to give long tubes which can be seen growing well down the style at the bottom of the photograph. Others, especially on the left, have produced only short tubes. The former are 'compatible' grains whose tubes can achieve fertilization. The latter are 'incompatible' and will never fertilize an egg nucleus. All pollen is incompatible on stigmata of the same flower. Pollen from other plants may be wholly incompatible, half compatible (as in the illustration), or wholly compatible. Thus self-fertilization can never occur, while cross-fertilization can.*

o (*below*) – (*right*) *Pollen from thrum and* (*left*) *pin flowers of the heterostyled* mula obconica. *The former is larger and less variable than the latter. It was t heterostyly acted as a mechanical means of promoting cross-pollination, the thrums touching those parts of a bee on which pin stamens had previously placed vice versa. Darwin, however, showed that this was not so, as he found that pollen o types was different. Pin pollen will not grow down a pin style, nor thrum n a thrum style, even when such 'illegitimate' pollinations are deliberately made. ological difference is often accompanied by a morphological difference as shown graph.* (× 500.)

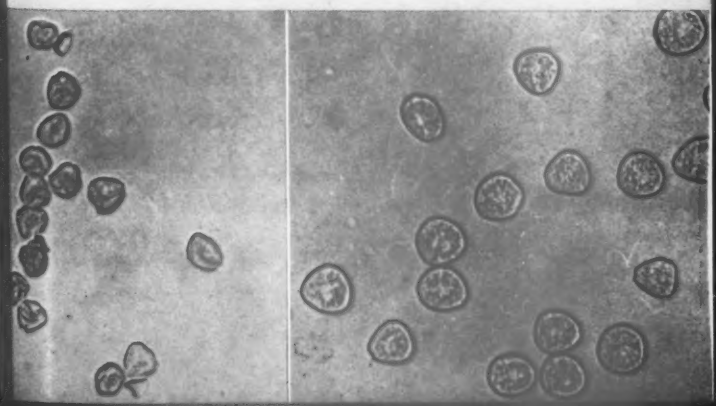
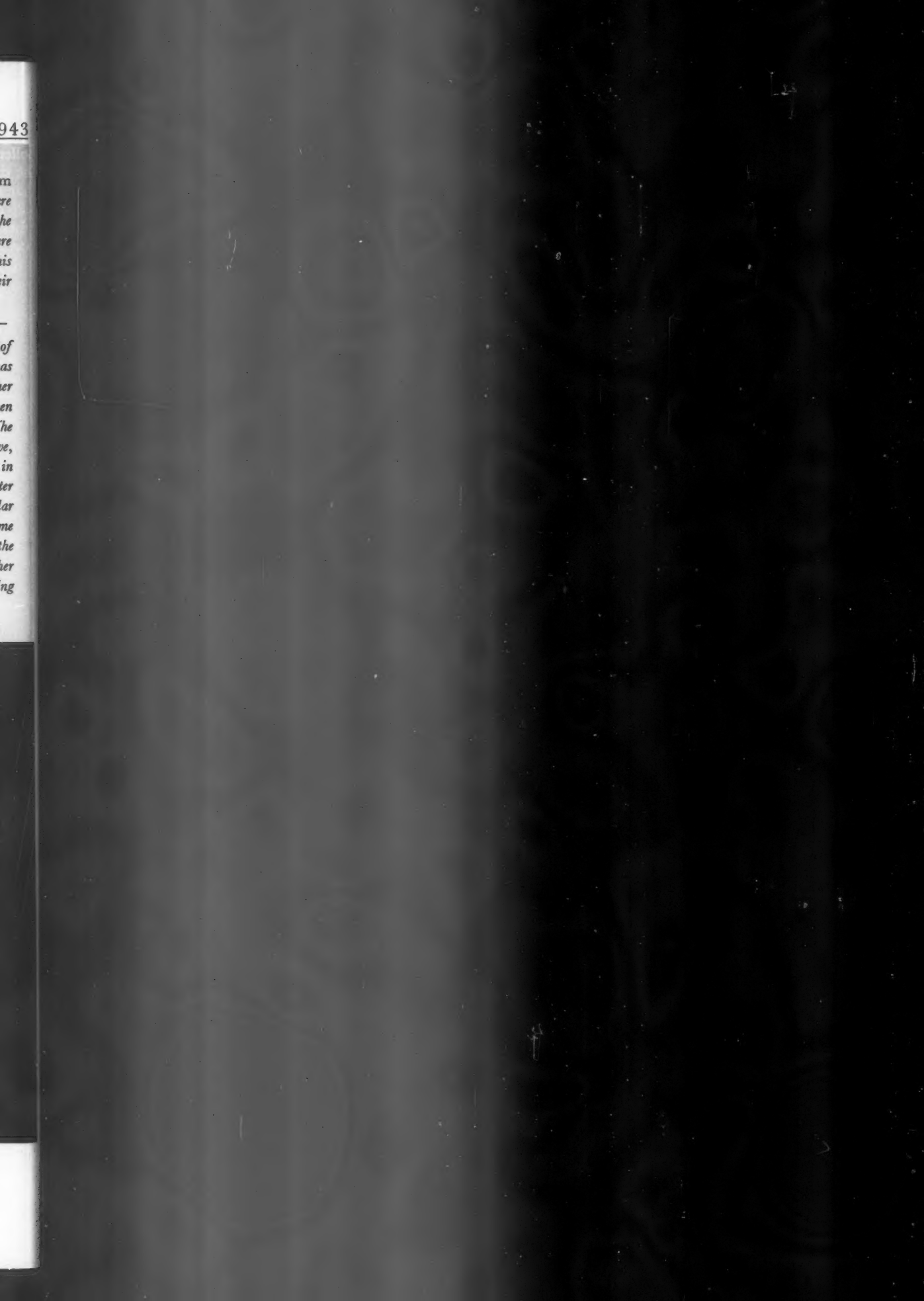


FIGURE 9 (*below*) – *Heterostyly in* Linum perenne. *The petals and sepals have been removed for clarity. On the left is a flower of the 'pin' type with long styles and short stamens. On the right is a flower of the 'thrum' type with short styles and long stamens. Note the pollen grains being released from the anthers and also present on the stigmata. Many plants, including primroses, show heterostyly.*

FIGURE 11 (*left*) – *A flowering spike of the couch grass* (Agropyrum repens). *Grasses are adapted to wind pollination. The anthers are extruded and swing in the wind, which then blows their pollen on to the feathery stigmata, also protruding from the flowers. These stigmata are most easily seen in the lower spikelets of the photograph. In spite of this mechanism, however, many grasses are regularly self-pollinated, for their anthers burst and pollinate the stigmata before either are extruded.*

FIGURE 12 (*below*) – *Single florets dissected from a flowering head of Scorzonera* (Scorzonera hispanica). *The youngest flower, on the left, has not yet opened. The next shows how the stigma pushes pollen out of the anther tube, visible just above the strap-shaped corolla. The pollen can be seen adhering to the outer, non-receptive, surface of the style and stigma. The stigma then opens out into two lobes, thereby exposing its inner, receptive, surfaces, much as in the willow-herb. These lobes curl over, as shown in remaining flowers, until the inner surfaces pick up pollen from the outer surface, so bringing about self-pollination. Most Compositae show similar behaviour. In spite of this, however, self-pollination is ineffective in some Compositae, e.g. the dahlia, for an incompatibility reaction, of the* Oenothera organensis *type, is operative. In the dandelion, on the other hand, pollination is not necessary for seed setting, reproduction being subsexual.*



pollen fails to reach suitable stigmata. Success at the time of pollination, on the other hand, means that both the seed parent and the pollen parent will be well represented in the next generation, other things being equal. Thus control of pollination carries a great selective advantage, and, as a consequence, many adaptations to this end are found in flowering plants.

The first man to realize the part played by pollen in sexual reproduction seems to have been Sir Thomas Millington, one of the original Fellows of the Royal Society, in the seventeenth century. But the most remarkable feature of pollination was not appreciated until nearly a hundred years later. Pollination can presumably be accomplished most easily and economically by the transference of pollen from the anthers to the stigmata of the same flower. In such a case the flower need not even open and its generative parts would thus be less exposed to damage by the elements. This is, in fact, exactly what happens in the small inconspicuous cleistogamous flowers of the violet. Yet, in spite of these advantages, it was observed that in many species bees are continually engaged in transferring pollen from one flower to another, often on a different plant. Cross-pollination must clearly offer some great advantage over self-pollination to compensate for the extra hazards which it involves.

These hazards are reduced in many plants by special floral mechanisms, which also assist in mate discrimination. Darwin (1877) realized the importance of floral adaptations for his evolutionary theories and subjected some of them to experimental analysis, which fully verified their apparent function in reproduction.

Since flowering plants are non-motile, they must employ external agents for the dissemination of their pollen. Wind, water, birds, insects, and even, it is stated, snails all play their part with different plants, whose flowers show most remarkable adaptation to the use of the particular agent in question. Caution is, however, necessary in interpreting floral structures in terms of pollination adaptation, for many plants do not now rely on the method which their flowers suggest. Grasses, for example, are well adapted to cross-pollination by wind, yet many of them, including wheat, oats, and barley, are self-pollinated in most instances. The crossing mechanism is vitiated by the anthers bursting before they are extruded from the flower. Similarly many of the Compositae show a peculiar curling of the stigma lobes, in such a way that self-pollination is brought about. Yet all these plants have showy flower-heads of a kind usually associated with the attraction of insects for cross-pollination. Furthermore, in some Compositae, like the dahlia, pollen will not germinate on the stigmata of flowers of the same plant; and dandelions do not even require pollen for seed setting, in spite of their floral structures, their reproduction being subsexual.

These mixed control mechanisms must be regarded as relics of different stages in the evolution of the species (Mather 1942). Sometimes cross-breeding has been advantageous, but at others self-pollination has been more conducive to success. Since evolution is not reversible, each change has superimposed a new mechanism on the older ones, and we now observe the relics of these various steps in the history of the species.

Most plants are cross-pollinated, but some, like wheat and the tomato, regularly show self-pollination. Hence self-pollination is not of itself deleterious. In cross-breeding plants, as Darwin (1876) showed, self-pollination leads to few and weak offspring; but this is not true of the normally self-pollinating species. Their genetical structure is fitted to self-pollination. The breeding system is simultaneously determined by, and in control of, the genetical structure of the species. A change in one brings about a change in the other, and the breeding mechanism of a species can alter when conditions render it advantageous.

Self-pollination, by causing in-breeding, leads to high adaptation of individuals to the local environment. It reduces the frequency of variant, and hence less fit, individuals. But in-breeding also lowers the chance of prospective adaptive change because it leads to a less flexible genetical structure. Cross-breeding, on the other hand, gives great genetical flexibility though at the expense of a reduction in immediate adaptation and fitness (Mather, 1942). The breeding system of a species is the breeding system which, in its ancestors, most successfully balanced the discordant advantages of fitness and flexibility under the changing environments of the past. Thus the survival of a species, and indeed of its descendants, depends on the adequacy of its system of breeding, or, as we may call it, mating discrimination.

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The development and use of luminescent substances

LEONARD LEVY and DONALD W. WEST

Substances that shine in the dark have for centuries excited scientific no less than popular curiosity. During the last decade noteworthy advance has been made, not only in our theoretical knowledge of luminescent compounds but in their practical application. Dr Levy and Mr West, who have for many years specialized in the study of these interesting substances, describe the principal results and conclusions hitherto obtained.

Luminescent substances are materials capable of emitting light without the simultaneous emission of a sensible amount of heat. This definition includes types of luminescence which are not of any technical importance, such as self-luminous living organisms. It also includes chemi-luminescence, which is chemical action accompanied by the emission of light but by only slight emission of heat.

The most important class of luminescent substances, however, consists of materials capable of converting various types of invisible radiation into visible radiation. Many naturally occurring minerals display luminescent effects of this nature, but in practically no instance are they in any way comparable in performance with chemically prepared luminescent materials, now known as 'phosphors.' The term 'phosphor' is by convention restricted to artificially prepared materials displaying intense luminescent effects, in which the final stage of preparation is that of crystallization by heating to a high temperature with suitable fluxes.

Certain pure crystalline salts display intense luminescence without being heated, the most important of them being the platinocyanides and certain uranium salts. These are not, however, usually included in the term 'phosphor.'

Luminescence affords a very good example of a phenomenon which, until comparatively recently, was of scientific interest only, but which has now become of great practical importance. In fact, the application of certain phosphors will offer an important contribution to the amenities of peace-time civilization; and at the present time many are of great importance in connection with the war effort.

The first recorded example of a phosphor is the so-called 'Bologna phosphorus,' made in 1692 by a cobbler of Bologna, who prepared a crude form of barium sulphide by heating heavy spar to a high temperature, probably in the presence of some reducing agent. This substance glowed in the dark after exposure to light.

X-ray screens, first used in 1895, and the self-luminous mixture of zinc sulphide phosphor and radio-active matter (such as is used on watch dials) were almost the only practical applications of luminescence until recently. The really great development of luminescent materials has occurred during the last ten years, chiefly in connection with cathode-ray tubes for television and other purposes, and for fluorescent lighting.

Phosphors vary greatly in the character of their luminescence, and their individual properties determine their practical application. These characteristics are perhaps most easily appreciated after a study of figure 1, which shows (C–D) the relationship between the intensity of the luminescence and the time which has elapsed since the incident exciting radiation has ceased.

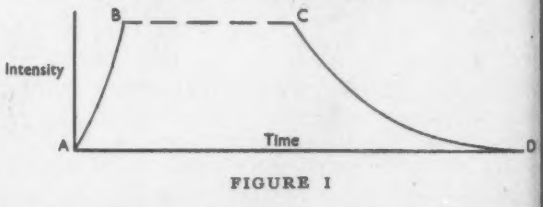


FIGURE 1

On excitation, there is a period of time, which may vary between a small fraction of a second and about half a minute (depending on the material and the intensity of the exciting radiation), during which the light emitted increases to a maximum. This is shown by the portion of the curve A–B. Saturation then occurs and the intensity of the emitted light remains constant until the exciting radiation ceases; this portion

of the curve is represented by B–C. In certain instances, as for example the action of alpha radiation on zinc sulphide phosphors, this portion of the curve is not horizontal, as the luminescent properties of the phosphor are progressively destroyed over a period of time. In many instances, however, it does remain horizontal. After the exciting radiation has ceased, the luminosity falls away according to the curve C–D. The luminosity may disappear in a small fraction of a second, or it may persist for many hours.

The emission of light during excitation, as represented by the portion of the curve B–C, is known as fluorescence; whereas the emission of light after incident radiation has ceased, represented by portion C–D, is known as phosphorescence. The latter obviously represents latent energy, stored in the phosphor, which is emitted more or less rapidly in the form of light. The intensity of the phosphorescence decays, and the time-intensity characteristics of the phosphorescence curve C–D are usually of great practical importance.

Phosphors show great variety in their response to different types of radiation. Some, as for example the zinc sulphide and zinc-cadmium phosphors, give excellent results when excited by almost every form of radiation except ultra-violet 2537 A, but other phosphors respond to one or two types of radiation only. The response of the more important types of phosphor to various types of radiation are shown in Table 1.

TABLE I

	Long ultra-violet 3650 A	Short ultra-violet 2537 A	X-rays	Cathode-rays	Alpha-rays from radio-active sub-stances
Zinc and zinc-cadmium sulphides	Very good	Slight to fair	Very good	Very good	Very good
Cadmium and magnesium tungstates	Almost nil	Very good	Good	Good	Very slight or nil
Zinc and zinc-beryllium silicates	Very slight	Very good	Fair	Very good	Very slight or nil
Cadmium borate	Poor	Very good	Very slight	Moderate	Very slight or nil
Cadmium and zinc phosphates	Poor	Very good	Very slight	Good	Very slight or nil

The luminescence can be varied by suitably modifying the method of manufacture of the phosphor; for example, cadmium tungstate, which displays a good bluish fluorescence under X-rays, is quite unaffected by long-wave ultra-violet. If, however, a small quantity of uranium is added the resulting phosphor displays pink fluorescence under X-rays and a vivid red fluorescence of great intensity under long-wave ultra-violet.

Experience has shown that in order to produce phosphors of the highest possible efficiency, certain procedures must be rigidly adhered to. The phosphor must be prepared, by precipitation or otherwise, in an absolutely pure condition. Metals such as iron, vanadium, and cobalt must be completely absent—their presence, even in the most minute traces, exerts a very prejudicial effect. The production of phosphors is more akin to bacteriological than to chemical work in this respect. Generally speaking, certain additives must be included in the phosphor to enhance the luminescent phenomena. The amount of additive required is often quite small, of the order of 1 part in 20,000. In other types of phosphor the amount is comparatively large, say 0·25–5 per cent. The additives are usually known as activators. The phosphor precipitate, together with its activator, is mixed with certain fluxes, which are varied according to the purpose for which the phosphor is intended, and the mixture is then heated to a high temperature, to cause the phosphor to crystallize. In some cases (e.g. calcium tungstate) the phosphor precipitate may be sufficiently crystalline to display some luminescent properties itself. The intensity of this luminescence is, however, very much lower than it is after heating.

Phosphors can be divided into three classes:

(1) The type exemplified by zinc and zinc-cadmium sulphides, in which the activator is present in minute quantities, usually not more than 1 part in 10,000.

(2) The type exemplified by zinc and zinc-beryllium silicates, in which the activator (manganese) required to produce luminescence is present in relatively large quantities, of the order of 0·5 per cent.

(3) Phosphors which are apparently absolutely pure salts and do not require the presence of any activator. This type is exemplified by the tungstates of magnesium, cadmium, and calcium. Even in this class, however, the colour of the luminescence can be modified by small additions. For example, calcium tungstate exhibits a blue fluorescence, but the addition of about 1 per cent. of samarium changes the colour to pink.

It has already been mentioned that the portion of the curve in figure 1, C–D, representing phosphorescence, is often of great importance. In certain instances it is necessary that zinc and zinc-cadmium sulphide phosphors should exhibit fluorescence without any phosphorescence, and it has been found that the presence of a small trace of nickel, of the order of 1 part in 1,000,000 or even less, reduces the phosphorescence to an extremely low level without materially affecting the fluorescence. This is a most remarkable fact and appears to be a unique characteristic of the metal nickel.

The spectra of phosphors are generally continuous bands of radiation extending sometimes into the ultra-violet and infra-red regions of the spectrum. Sometimes, as in calcium tungstate activated with samarium, a certain amount of fine structure is present also. The band sometimes extends over the whole visible spectrum with one or two well-defined peaks. In other cases the bands are relatively narrow and the resulting colour is much more saturated. Examples of fluorescence spectra are shown in figure 2.

Intensities of luminescence vary very greatly, from small luminosities as in the case of radio-active luminous compound of low quality as used for watch dials, etc., to luminosities of enormous intensities as exhibited by the scanning spot in television tubes. Table 2 shows the approximate values of certain fluorescence intensities.

TABLE 2

Phosphor	Excitation	Intensity
ZnCdS(Ag)	X-rays, 6 ma. 90 kv.	5 equivalent foot-candles
ZnS(Ag) plus ZnCdS(Cu)	Cathode-ray scanning spot in television tube	About 6,000,000 equivalent foot-candles
ZnS(Cu)	α-rays (70 micrograms per gram)	0·04–0·05 equivalent foot-candles
ZnS(Cu)	Ultra-violet, 125 watts at 3 ft.	6 equivalent foot-candles
SrS(Bi)	Daylight	1·5 equivalent foot-candles

Fluorescence is explained in terms of modern atomic physics in the following manner. The electron is supposed to be raised from a lower or filled band in the lattice to the conduction band by the absorption of radiation. The activator supplies electrons to replace those excited from the lower band. Emission of light takes place when the electron falls from the conduction band to the impurity band.

Phosphorescence is explained by postulating the presence of metastable bands (the so-called 'trapping-levels') close to the conduction band. The electrons in the trapping-levels can be released only via the conduction band, owing to thermal oscillations.

The first really practical use for luminescent substances was for X-ray diagnosis. Screens coated with luminescent materials were used for converting X-rays into visible radiation, thus enabling visual examinations to be carried out. A different type of screen is also used for converting X-radiation into actinic radiation, thus very greatly reducing the exposure necessary when making a radiograph. Phosphors for X-ray work should be free from phosphorescence, as otherwise a blurred image of moving organs will be seen. It was this which prevented the use of zinc sulphide for X-ray work for many years. Zinc and zinc-cadmium sulphide phosphors give far more intense fluorescence under X-rays than other substances, but always exhibit phosphorescence, which rendered them quite unsuitable. The discovery of the 'nickel killer,' already referred to, removed this difficulty.

Radio-active luminous compound has been known for quite a long time. It has been in general use for clock and watch dials, but a very much higher grade is now used for luminizing the dials of aeroplane instruments, ships' compasses, and other purposes. It is a curious fact that only one phosphor is used for this purpose, zinc sulphide activated with copper. It responds to alpha-radiation, which constitutes nearly all the energy of radio-active radiations.

Zinc and zinc-cadmium sulphide phosphors are employed in conjunction with long-wave ultra-violet radiation for display purposes for advertising and stage effects, and they have also

found considerable application since the war for blackout illumination. They afford a simple and easy method of obtaining low levels of illumination without the use of electrical fittings and supplies.

Sulphides, silicates and tungstates and certain other phosphors all find application for use in cathode-ray tubes for various purposes. The great and rapid development in the use of cathode-ray tubes has led to a concomitant increase in the number and variety of the phosphors required to produce certain special effects. The screen in cathode-ray tubes used for instruments such as oscillographs and the like is usually made of zinc silicate giving a green fluorescence. The screen in television cathode-ray tubes is required to give an image which is substantially black and white. This is not well achieved by any single phosphor, which, even if its spectrum is continuous, always shows a predominance in certain portions. Very good results are obtained by mixing together zinc-cadmium sulphide, giving orange and orange-yellow fluorescence, with zinc sulphide giving a blue fluorescence which extends back into the green portion of the spectrum. The fluorescence spectrum of a mixture of these two phosphors is shown in figure 2, and it will be seen that they are roughly complementary, the two together making up practically the whole of the visible spectrum. Cathode-ray tubes with special phosphors have found a great many new applications during the present war, for example in radiolocation and in new methods of gun-laying.

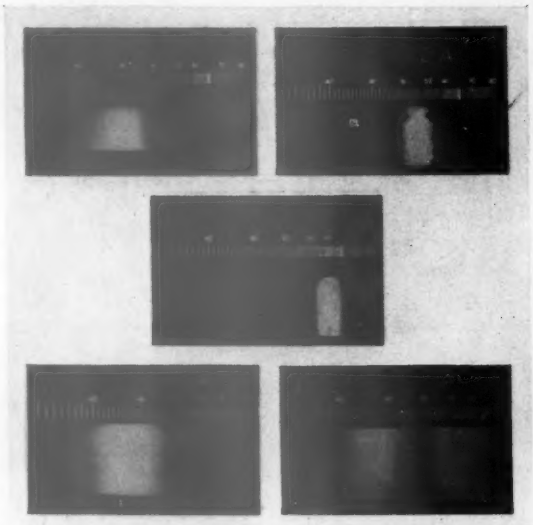


FIGURE 2 – *1A, cadmium tungstate; 2A, zinc silicate; 2A2, zinc phosphate; 4B, zinc sulphide (Ag); 3C, zinc sulphide and zinc-cadmium sulphide mixture.*

Phosphors are principally used for the fluorescence which they exhibit. In certain cases, however, it is the phosphorescent and not the fluorescent effects which are desired. Thus strontium sulphide phosphors have found such use in the present war for black-out signs of various kinds. Unfortunately the alkaline earth phosphors are chemically unstable and require the use of special media to prevent their decomposition. In the past these phosphors have been employed in media which were totally unsuitable, with the result that their luminescent qualities speedily deteriorated.

A second type of phosphorescent phosphor is zinc sulphide activated with copper. This exhibits a very intense phosphorescence which, however, dies away far more rapidly than that of the alkaline earth phosphors. The intensity curves of the two types of phosphors cross in a few minutes, after which the phosphorescence of the strontium sulphide is always higher than that of the zinc sulphide. Zinc sulphide phosphors are, generally speaking, very stable. They require suitable media to prevent photolysis, but are not readily decomposed chemically. They can in fact be incorporated in suitable frits to yield phosphorescent enamels. These enamels are very useful in cases of complete failure of the electric lighting, as they afford sufficient light to enable objects to be discerned in an otherwise unlighted room. Black-out conditions are of course particularly favourable for the use of phosphorescent phosphors, which can be incorporated in the synthetic resins employed for switch covers, door handles, etc. In order to make such articles of widespread utility it would be necessary to effect further improvements in the phosphorescence.

By far the largest and most important application of luminescent powders is undoubtedly in illumination. The first discharge tubes to utilize luminescent powders were the familiar high voltage neon and mercury-filled tubes used for advertising, but similar tubes were also extensively developed in this country for general lighting purposes. The technique used for the high voltage fluorescent tubes has more recently been widely applied to similar tubes operating directly from ordinary supply voltages, and these mains-voltage fluorescent tubes bid fair to revolutionize artificial light sources.

FIGURE 1 – *Mule-spinners' cancer: the primary lesion.*

FIGURE 2 – *Papillomatosis of rabbit's ear, induced by the repeated application of a tar prepared from acetylene (Kennaway).*

FIGURE 3 – *Fluorescence spectrum of 3 : 4-benzpyrene (Hieger). Note sharply defined edges of the two main fluted bands.*

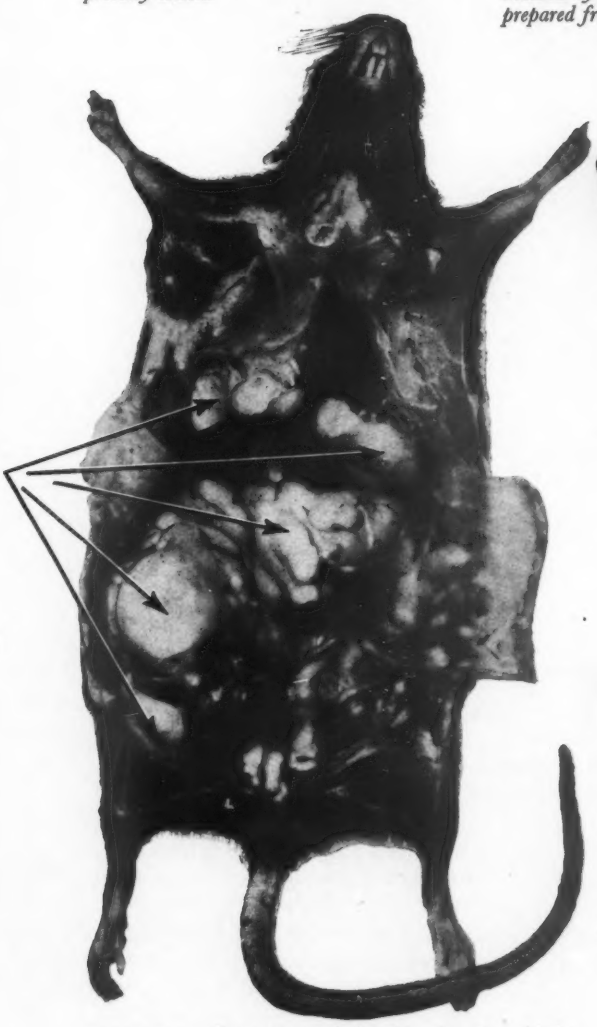


FIGURE 5 – *Rat sarcoma, induced by the repeated injection of 1 : 2 : 5 : 6-dibenzanthracene. The main tumour-masses are indicated by arrows.*

FIGURE 4 – *Epithelioma (mouse), induced by painting the skin with a benzene solution of 3 : 4-benzpyrene.*

FIGURE 6 – *Invasion of the liver by a malignant connective-tissue tumour (fowl), induced with 1 : 2 : 5 : 6-dibenzanthracene.*

FIGURE 7 – *Rapidly dividing malignant cells from a tumour (of mouse skin) induced with 10-hydroxymethyl-1 : 2-benzanthracene.*

The production of cancer by chemical compounds

ALEXANDER HADDOW

Until comparatively recent times the cure of disease was a matter of trial and error, but the rapid development of science has revolutionized experimental medicine. Dr Haddow's article describes the application of chemical knowledge to the investigation of cancer, and affords a striking example of one aspect of the new technique.

The history of carcinogenesis illustrates, with a wealth of examples, the dependence of accident and planned experiment.

The tale began when Percivall Pott described a particular form of cancer occurring in chimney-sweeps (cancer scroti, in *Chirurgical Observations*, 1775) and traced it to a specific cause, namely contamination of the skin by soot. Other cases were reported by a long line of English surgeons, including Earle, Astley Cooper, Hughes Bennett, Paget, and Butlin. In the latter part of the nineteenth century appeared three new and powerful stimuli to investigation, in fresh accounts of cancer as an industrial hazard. Thus in 1875 von Volkmann discovered occupational skin tumours among the workers in the tar industry at Halle. In the following year the celebrated Joseph Bell of Edinburgh described the earliest cases of 'paraffin cancer' in the Scottish shalefield. In 1887 was found the first example of skin cancer affecting operatives in the Lancashire cotton-spinning industry and attributable to contact with lubricating oil ('mule-spinners' cancer') (*see* figure 1).

All these were the results of undesigned 'natural' experiments. Answers to the questions they posed could be found only by systematic inquiry, and the first essential lay in the reproduction of the disease at will. One of the earliest attempts was made by Hanau in 1889. Inspired by von Volkmann's paper, he tried to induce cancer in rats and dogs by painting the skin 'mit Hallenser Braunkohlentheer,' but without result. Success proved tardy, just eluded Bayon in this country (1912), but was finally vouchsafed to Yamagiwa and Ichikawa (1915), who produced undoubted cancers by application of coal-tar to the ears of rabbits. In 1918 Tsutsui (a pupil of Yamagiwa) showed the advantage of painting the skin in *mice* as a method of testing—a simple technical variation but one which influenced all later developments most profoundly.

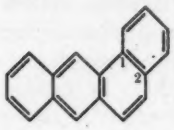
The scene was now set for a wider attack to elucidate the nature of the cancer-producing agent, and the instrument had already been described by the renowned Lebert (1851), in prophetic advocacy of a chemical approach: '*Il faut que, pour un pareil travail, un homme profondément versé dans la connaissance anatomique et pathologique du cancer se réunisse à un chimiste qui, de son côté, soit au courant des progrès les plus récents de la science.*'

The first-fruits of this union came in 1921–26, with a demonstration by Bloch in Zürich that the active substance of coal-tar was concentrated in the higher boiling fractions as a neutral compound, free from nitrogen, arsenic, or sulphur, capable of forming a stable complex with picric acid, and probably belonging to the class of cyclic hydrocarbons.

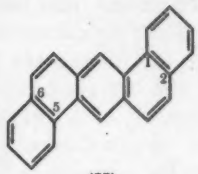
In 1924–25 Kennaway succeeded in producing carcinogenic tars by the pyrolysis of petroleum, skin, hair, yeast and cholesterol, and by leading acetylene and isoprene with hydrogen through strongly heated tubes. The tars obtained at successively higher temperatures (450–1,250° C.) showed an ascending order of potency; and these results could be explained by assuming that acetylene was a common decomposition product, and that the carcinogenic material was actually formed from acetylene (*see* figure 2). There gradually emerged the strong general conclusion that the carcinogenic agent was a complex hydrocarbon of aromatic type.

Since the carcinogenic tars usually showed strong fluorescent properties, Mayneord (1927) examined their fluorescence spectra and discovered them to possess a characteristic spectrum consisting of three bands at low dispersions. This

vital clue was developed by Hieger in an investigation of a number of polycyclic aromatic hydrocarbons, particularly those related to anthracene. 1 : 2-*Benzanthracene* (I) was found to give a spectrum similar to that of the carcinogenic mixtures, and it was this which led Cook to embark on the long series of studies of benzanthracene homologues which founded our knowledge of the structural conditions underlying carcinogenic action. About this time Clar had just published methods for the synthesis of certain benzanthracene hydrocarbons. These compounds were tested for carcinogenic activity by Kennaway and Hieger (1930), who soon obtained positive results with 1 : 2 : 5 : 6-dibenzanthracene (II); this was, therefore, the first known compound manifesting pronounced carcinogenic properties (figures 5 and 6).

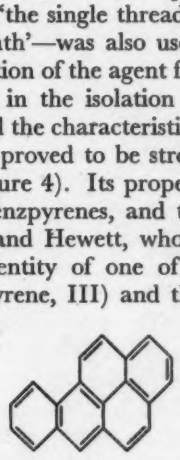


(I)
1 : 2-*Benzanthracene*



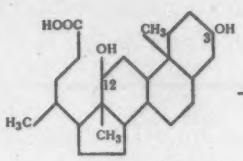
(II)
1 : 2 : 5 : 6-*Dibenzanthracene*

The fluorescence spectrum—in Kennaway's words 'the single thread that led all through this labyrinth'—was also used by Hieger in the concentration of the agent from coal-tar pitch, culminating in the isolation of a hydrocarbon which showed the characteristic spectrum (figure 3), and which proved to be strongly carcinogenic (1933) (*see* figure 4). Its properties suggested one of the two benzpyrenes, and these were synthesized by Cook and Hewett, who were soon able to prove the identity of one of the compounds (3 : 4-benzpyrene, III) and the substance from pitch.

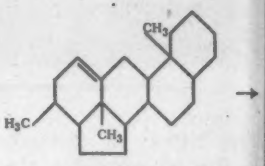


(III)
3 : 4-*Benzpyrene*

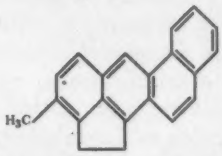
Thus far the clues drawn from industrial cancer had led to the identification of the substance responsible, and to the discovery of other hydrocarbons related to benzanthracene and possessing carcinogenic properties. The possibility that such compounds might play a part in the causation of 'spontaneous' cancer was opened up by recognition, following the advances in sterol formulation due to Rosenheim and King in 1932, that many naturally occurring compounds have somewhat similar condensed polycyclic systems. Thus dehydronorcholene (V), a hydrocarbon prepared by Wieland and Schlichting from the bile-acid deoxycholic acid (IV), is a hydrogenated derivative of 1 : 2-benzanthracene, and in 1933 both Wieland and Dane, and Cook and Haslewood, obtained from it, by dehydrogenation with selenium, the fully aromatic hydrocarbon methylcholanthrene (VI).



(IV)
Deoxycholic acid



(V)
Dehydronorcholene



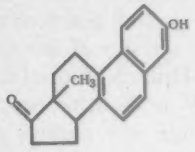
(VI)
2-o-*Methylcholanthrene*

This compound soon proved to have carcinogenic potency of a high order—as Cook had indeed predicted. The structure was rapidly established and confirmation provided by its synthesis (Fieser and Seligman).

There is as yet no proof that changes of the kind envisaged take place *in vivo*. Both cholic acid and deoxycholic acid have a hydroxyl group in such a position (12) as would promote the necessary cyclization of the side-chain. On the other hand, Rossner (1937) obtained methylcholanthrene by dehydrogenation of complex hydrocarbazole derivatives formed by the interaction of cholestenone or cholestanone with phenylhydrazine. In none of these cases is there any special circumstance to favour ring-closure, and hence Cook regarded this result as suggestive of an inherent tendency for the sterol molecule to pass into a methylcholanthrene derivative.

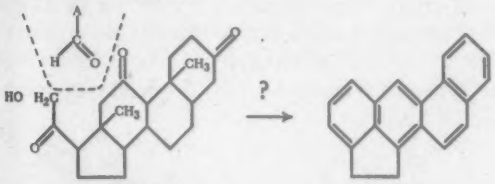
However, marked carcinogenicity is not generally found in compounds possessing hydrogenated rings, so that a further stage of dehydrogenation would be required. Important here is a finding by Ghiron (confirmed by Kennaway) that injection of deoxycholic acid itself may produce connective-tissue tumours in mice; in this case it will be particularly desirable to find whether deoxycholic acid is intrinsically active, or is subject to a transformation, yielding traces of an active

product. One of the best analogies for aromatization of the hydrogenated ring system is provided by the oestrus-producing hormone equilenin (VII), possessing two aromatic rings and probably arising from a non-aromatic steroid precursor (Girard, Fieser).



(VII)
Equilenin

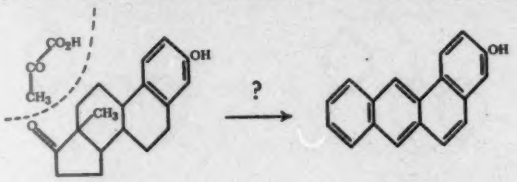
Fieser has recently reviewed newer opinions on the relationships between the steroids, dealing with Robinson's suggestion of a scheme involving the biosynthesis of steroids from tyrosine, and with the hypothesis of Reichstein, according to which the synthetic units may be three-carbon sugars. In the latter process, condensation of seven units would afford the cortical steroids (C_{21}) and could proceed further to give the bile-acids (C_{24}) and cholesterol (C_{27}), the C_{19} androgens and C_{18} oestrogens being assumed to arise by metabolic degradations of C_{21} steroids. In Fieser's view the rough analogy between equilenin and methylcholanthrene suggests that, if the latter does arise by a process of abnormal metabolism, this process would be associated with the normal one which results in the production of oestrogenic hormones. He therefore invites consideration of the production of carcinogens from the principles of the adrenal cortex, visualizing such reactions as might ensue from the condensation of dehydrocorticosterone (VIII) with formaldehyde (to give cholanthrene, IX).



(VIII)
Dehydrocorticosterone
(plus an aldehyde)

(IX)
Cholanthrene

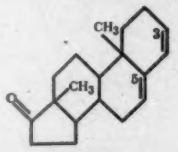
He also suggests another route to a cholanthrene in the condensation of oestrone (X) or equilenin with pyruvic acid, followed by aromatization, ring-closure to give the terminal benzenoid nucleus, and further dehydrogenation. A difficulty arises here (as also for deoxycholic acid, above), since the product, on account of the persistence of the hydroxyl group at C_3 characteristic of partially aromatic steroid hormones, would be a 3-hydroxy derivative (XI) and unlikely to be carcinogenic. A highly relevant observation was, however, made by Burrows, Cook, Roe, and Warren when they isolated from the urine of a male with an adrenal tumour a substance (XII) lacking this characteristic 3-hydroxyl group, and so showing that the C_3-hydroxyl may in certain circumstances be eliminated while the ring-system is still non-aromatic. The same compound



(X)
Oestrone
(plus pyruvic acid)

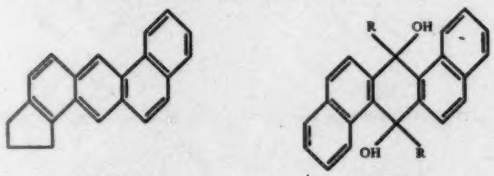
(XI)
3-Hydroxycholanthrene

was later isolated by Wolfe, in Fieser's laboratory, from the urine of a female with adrenal tumour.



(XII)
$\Delta^{3,5}$-Androstadiene-17-one

Mention may here be made of a discovery by Lacassagne in 1932, that injection of the female sex hormone produces, in favourable conditions, cancer of the breast in male mice—the first experiment in which cancer was produced by a naturally occurring compound. About the same time Cook and Dodds showed that individual synthetic compounds, e.g. 5 : 6-*cyclo*penteno-1 : 2-benzanthracene (XIII) and 3 : 4-benzpyrene (III), might show oestrogenic as well as carcinogenic activity; and oestrogenic action was also discovered in certain di-alkyl derivatives of 1 : 2 : 5 : 6-dibenz--9 : 10-dihydroanthraquinol (XIV).

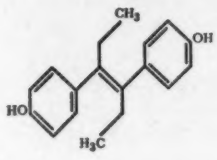


(XIII)
5 : 6-cyclo-*Penteno-*
1 : 2-*benzanthracene*

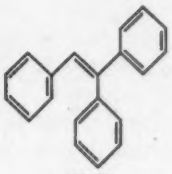
(XIV)
Di-alkyl-dibenzanthracene diols

This soon led to larger scale researches by Dodds and Robinson and to the synthesis of artificial oestrogens of extraordinary potency.

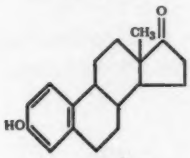
Among such compounds discovered later, diethylstilboestrol (XV) and triphenylethylene (Schönberg) (XVI) are now known to be capable of inducing breast and testis tumours in mice of susceptible strains. The former compound presents a rough resemblance to oestrone (XVII) and to chrysene (XVIII), and triphenylethylene may also be formulated as a phenanthrene derivative with one ring disrupted, or possibly as a benzanthracene system with two rings disrupted.



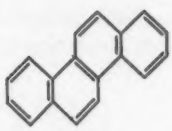
(XV)
Diethylstilboestrol



(XVI)
Triphenylethylene

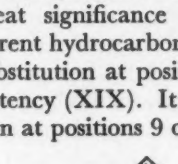


(XVII)
Oestrone

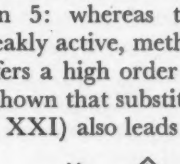


(XVIII)
Chrysene

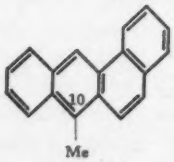
There has now accumulated a mass of data on the basis of which it is possible to relate molecular features and biological activity, and to illustrate a community of structure for the majority of carcinogenic compounds. Cook's study of the benzanthracene homologues first disclosed the great significance of position 5: whereas the parent hydrocarbon is only weakly active, methyl substitution at position 5 confers a high order of potency (XIX). It was later shown that substitution at positions 9 or 10 (XX, XXI) also leads to highly carcinogenic compounds, and indeed 10-methyl-1 : 2-benzanthracene is the most active of all the monomethyl derivatives of the parent compound. Of the remaining monomethyl isomerides, only the 6-methyl derivative exhibits notable activity, the others little or none.



(XIX)
5-Methyl-1 : 2-benzanthracene

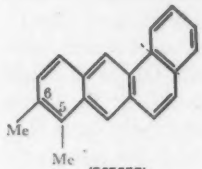


(XX)
9-Methyl-1 : 2-benzanthracene

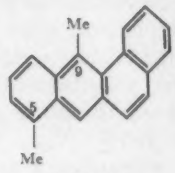


(XXI)
10-Methyl-1 : 2-benzanthracene

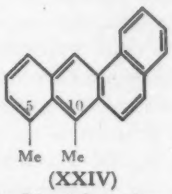
Proceeding to dimethyl-benzanthracenes, substituents at any two favourable positions reinforce each other, so that the activity of the new compound is enhanced. Thus 5 : 6-dimethyl-1 : 2-benzanthracene (XXII) is more active than the compound possessing only one methyl group at position 5 or 6, high activity is shown by 5 : 9- and 5 : 10-dimethyl-1 : 2-benzanthracene (Fieser) (XXIII, XXIV), and extreme potency is reached in 9 : 10-dimethyl-1 : 2-benzanthracene (Bachmann, Kennaway) (XXV).



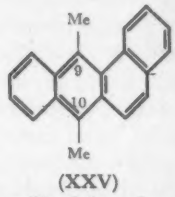
(XXII)
5 : 6-Dimethyl-1 : 2-benzanthracene



(XXIII)
5 : 9-Dimethyl-1 : 2-benzanthracene

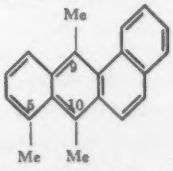


(XXIV)
5 : 10-Dimethyl-1 : 2-benzanthracene

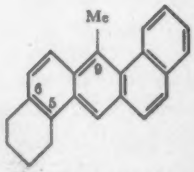


(XXV)
9 : 10-Dimethyl-1 : 2-benzanthracene

Still greater augmentation is effected by substitution simultaneously in *three* favourable positions, as in 5 : 9 : 10-trimethyl-1 : 2-benzanthracene (XXVI) and 9-methyl-1 : 2 : 5 : 6-dibenzanthracene (XXVII).



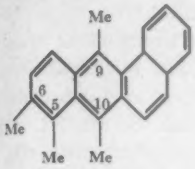
(XXVI)
5 : 9 : 10-Trimethyl-1 : 2-benzanthracene



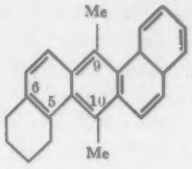
(XXVII)
9-Methyl-1 : 2 : 5 : 6-dibenzanthracene

But there is a limit to advance in potency by increase in substituted positions, since the compounds in which the four most favourable are

substituted [5 : 6 : 9 : 10-tetramethyl-1 : 2-benzanthracene (XXVIII) and 9 : 10-dimethyl-1 : 2 : 5 : 6-dibenzanthracene (XXIX)] show *decreased* activity as compared with XXVI and XXVII:

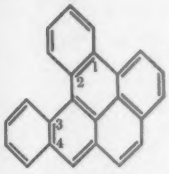


(XXVIII)
5 : 6 : 9 : 10-*Tetramethyl*-1 : 2-*benzanthracene*

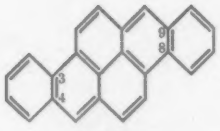


(XXIX)
9 : 10-*Dimethyl*-1 : 2 : 5 : 6-*dibenzanthracene*

This at once suggests the dependence of activity on optimal complexity, a conclusion which Cook had stressed at an early stage in these investigations, and which is upheld by many later examples. The limits of complexity compatible with activity are represented by two hexacyclic hydrocarbons, 1 : 2 : 3 : 4- and 3 : 4 : 8 : 9-dibenzpyrene (XXX, XXXI):

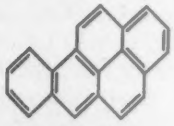


(XXX)
1 : 2 : 3 : 4-*Dibenzpyrene*

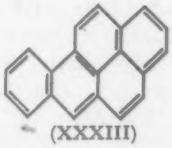


(XXXI)
3 : 4 : 8 : 9-*Dibenzpyrene*

These cases permit some degree of analogy with derivatives of 3 : 4-benzphenanthrene and chrysene, and remind us that while Cook regarded 3 : 4-benzpyrene as most nearly related to 1 : 2-benzanthracene (as in XXXII), for other reasons Fieser suggested it should more suitably be formulated as a chrysene derivative (as XXXIII):

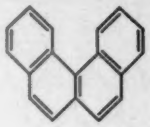


(XXXII)



(XXXIII)

Marked progress was achieved on complete examination of the six hydrocarbons consisting of four condensed aromatic rings, and of the fifteen compounds having five such rings. This revealed activity in 3 : 4-benzphenanthrene (XXXIV) and 1 : 2 : 3 : 4- and 1 : 2 : 5 : 6-dibenzphenanthrene (XXXV, XXXVI), and it also transpired that *chrysene*, as well as 1 : 2-*benzanthracene* and 3 : 4-*benzphenanthrene*, must be regarded as a parent compound of carcinogenic derivatives. Each of these hydrocarbons is a derivative of phenanthrene, substituted in two of the 1-, 2-, 3- and 4-

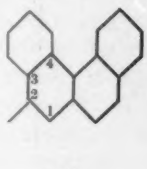


(XXXIV))
3 : 4-*Benzphenanthrene*

(XXXV)
1 : 2 : 3 : 4-*Dibenzphenanthrene*

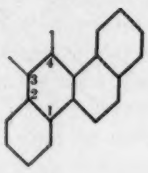
(XXXVI)
1 : 2 : 5 : 6-*Dibenzphenanthrene*

positions. Further substitution in the remaining positions gives rise to highly carcinogenic hydrocarbons (e.g. 2-methyl-3 : 4-benzphenanthrene (A), 1 : 2 : 3 : 4-dibenzphenanthrene (B), 1 : 2-dimethylchrysene (C), 3 : 4-benzpyrene (D), 9 : 10-dimethyl-1 : 2-benzanthracene (E). Moreover, substitution is effective whether by two benzene rings or by one benzene ring and one or two methyl groups: this finally led Hewett and Martin to the synthesis of 1 : 2 : 3 : 4-tetramethylphenanthrene (F)—a key compound which links the active derivatives of 1 : 2-benzanthracene, 3 : 4-benzphenanthrene and chrysene, may be regarded as the *prototype* of all these compounds, and has only recently itself proved carcinogenic.

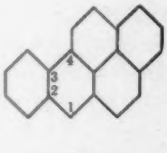


(A)

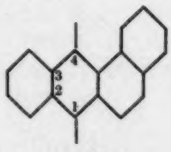
(B)



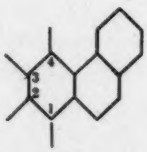
(C)



(D)



(E)

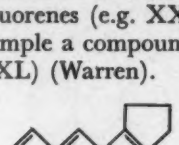


(F)

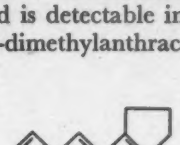
(Numbering indicates relation to phenanthrene)

Even the phenanthrene nucleus is not indispensable, since activity is marked in structures

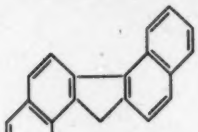
XXXVII and XXXVIII and in certain dibenzfluorenes (e.g. XXXIX), and is detectable in as simple a compound as 9 : 10-dimethylanthracene (XL) (Warren).



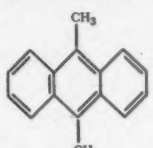
(XXXVII)
5 : 10-*Dimethylene*-1 : 2-cyclopentenoanthracene



(XXXVIII)
Δ^3-*Dehydro*-3 : 4-trimethylene-isobenzanthrene-2

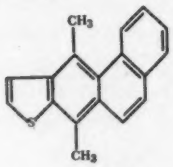


(XXXIX)
1 : 2 : 5 : 6-*Dibenzfluorene*

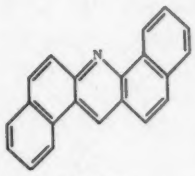


(XL)
9 : 10-*Dimethylanthracene*

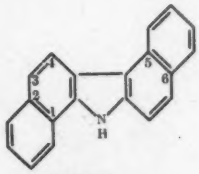
Nor is activity restricted to hydrocarbons, for it is found in a sulphur-containing compound (XLI) (Sandin and Fieser), and in such nitrogenous analogues as dibenzacridines and dibenzcarbazoles (e.g. XLII, XLIII). Boyland directed attention to 3 : 4 : 5 : 6-dibenzcarbazole, on account of its possible formation from β-naphthylamine, one of the intermediates thought to be responsible for cancer of the bladder in dye-workers, and now known, from experiments on dogs, to be able to induce this disease experimentally (Hueper, Bonser).



(XLI)
4 : 9-*Dimethyl*-5 : 6-*benzthiophanthrene*



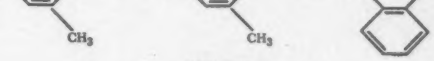
(XLII)
1 : 2 : 5 : 6-*Dibenzacridine*



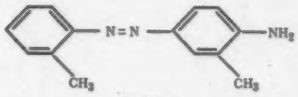
(XLIII)
1 : 2 : 5 : 6-*Dibenzcarbazole*

The substituent is also capable of variations compatible with activity, as in many compounds with oxygen-, nitrogen-, and halogen-containing groups, mostly at positions 10, 9, and 5 in 1 : 2-benzanthracene.

Carcinogenic potency is by no means confined to polycyclic compounds. Exceptional cases are provided by two styryl quinoline trypanocidal drugs (Browning) and by radio-active elements and their salts. But the great majority of those remaining fall in the class of *azo compounds*. As early as 1906 B. Fischer described epithelial proliferation following the injection of scarlet red (XLIV). Although the growths were non-malignant, Fischer rightly claimed his discovery as the first case of the induction of tumour-like proliferation by a chemical compound. A few years later, Hayward found that the active part of the scarlet red molecule was represented by 4′-amino-2 : 3′-azotoluene (XLV), and experiments with this and related compounds continued to be reported for a number of years.



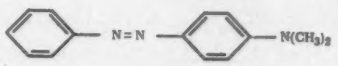
(XLIV)
'*Scarlet red*'



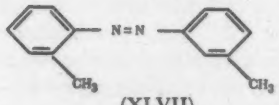
(XLV)
4′-*Amino*-2 : 3′-*azotoluene*

But little progress was made before a large-scale development by Yoshida (1931) when 4′-amino-2 : 3′-azotoluene was observed to produce proliferation of liver tissue in mice and liver tumours in rats.

Kinosita later examined other derivatives of azobenzene, and found an isomer of aminoazotoluene [*p*-dimethylaminoazobenzene (XLVI)] to be highly active in producing liver tumours. This substance, with 4′-amino-2 : 3′-azotoluene and 2 : 3′-azotoluene (XLVII), is also capable of



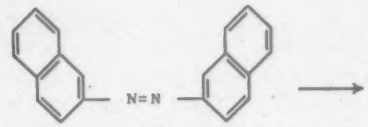
(XLVI)
p-*Dimethylaminoazobenzene*



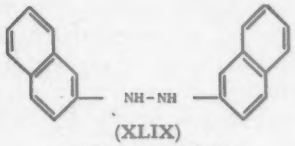
(XLVII)
2 : 3′-*Azotoluene*

producing connective-tissue growths. Yoshida also noted, in a few of his animals, papilloma formation in the stomach and bladder, and bladder tumours were later obtained with 2 : 3′-azotoluene.

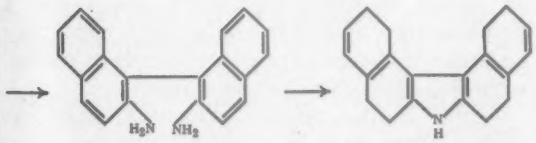
In other attempts to produce bladder cancer, Cook, Hewett and Kennaway selected the three *azonaphthalenes*, on the supposition that these might arise by oxidation of naphthylamines, and so be present in the crude bases which are a factor in the occupational incidence of the disease in chemical workers. No bladder tumours were obtained, but liver changes in mice, similar to those induced by the azobenzene derivatives, resulted from treatment with 2 : 2′-azonaphthalene (XLVIII). Azo compounds may be reduced to amines by way of the hydrazo compound (e.g. XLIX), and since this is susceptible to rearrangement, tests were made with the compound which might thus be expected, viz. 2 : 2′-diamino-1 : 1′-dinaphthyl (L).



(XLVIII)
2 : 2′-*Azonaphthalene*



(XLIX)
2 : 2′-*Hydrazonaphthalene*



(L) (LI)
2 : 2′-*Diamino*-1 : 1-*dinaphthyl* 3 : 4 : 5 : 6-*Dibenzcarbazole*

This induced liver tumours with even greater facility than 2 : 2′-azonaphthalene, a significant finding, since the diaminodinaphthyl readily undergoes deamination to 3 : 4 : 5 : 6-dibenzcarbazole (LI), which itself produces liver tumours, as well as skin cancer and malignant connective tissue tumours, in mice. These relations are obviously compatible with a suggestion that 2 : 2′-azonaphthalene is not inherently carcinogenic, but acts by a transformation to dibenzcarbazole; they thus provide some indication of a remarkable linkage between carcinogenic substances of the azo class and the polycyclic compounds.

The carcinogens are notable among chemical agents in producing a permanent transformation in the growth properties of cells. The chief feature of the new cell type is an increased *autonomy*, which shows itself by growth and invasiveness, independently alike of the further presence of the carcinogen or of the needs of the body as a whole. How is the change produced? From present evidence the essential influence of the carcinogen is to restrict normal cellular growth. The interference is characteristic in that no escape or easy acclimatization seems possible. At length, however, the biological response does occur, not as a continuous adaptation, but rather as a comparatively sudden break, which permits the cell to achieve independence in an environment still unpropitious for the normal form. Once emerged, growth of the new cell strain proceeds indefinitely, both in the same host and when carried through a series of healthy animals by transplantation—where it develops according to its new constitution and continues to exhibit an astonishing degree of constancy and specificity. As to the genetic nature of the change, many questions remain to be answered. Whatever the outcome, there can be no doubt of the fundamental character of these problems, transcending even their interest for medicine. There is, however, a need for more effectively integrating the subject with biology as a whole. When this is accomplished, the greatest contribution of such studies may yet be seen in the light they shed on cellular growth and variation in general, of which the problem of tumour induction is only a special part.

If we believe (with Rous) that the experimental pathologist must in the end express himself as surely as the artist, then here at least he has a prospect of success!

Surface tension and its measurement

ALLAN FERGUSON

There are many and serious gaps in the literature relating to surface tension. For example, there is a real need for systematic measurement of the relation between surface tension and temperature for the members of certain homologous series of pure organic compounds: such data would be of great theoretical and practical importance. Professor Ferguson shows that precise determinations of surface tension can be made with very simple apparatus.

The chief object of this article is to point out some of the gaps in our experimental knowledge of surface tension and to show how these gaps may be filled by experiments carried out with the aid of very simple apparatus, which apparatus is, however, capable of producing results having a high degree of precision.

That the surface separating a liquid from a gas (or the surface separating two immiscible liquids) behaves as if it were in a state of tension may be deduced from everyday observation; the spherical shapes of raindrops or of small globules of mercury, in which the 'flattening' effect of the force of gravity, which depends on the volume, is negligible compared with forces depending on surface effects, are cases in point. Nor are qualitative reasons far to seek. If we admit the existence of forces between molecules very great at small distances, but falling off very rapidly with increase of distance, we see that we may draw round any molecule in a liquid a *sphere of molecular attraction* whose radius is such that molecules within the sphere may be regarded as exerting sensible forces on the molecule at the centre, molecules outside the sphere exerting no appreciable force on the centrally situated molecule. It is clear that a molecule situated well within a liquid will, on the average, experience no resultant force due to the attractions of its neighbours. It is otherwise with a molecule in the liquid surface, or at a distance from the surface less than the radius of the sphere of molecular attraction; a molecule so situated will experience an inward force perpendicular to the surface. Hence, to bring more molecules into the surface—that is to extend the surface—demands the expenditure of work, and *to this extent* the surface behaves like a stretched elastic membrane and tends to contract. The analogy must not be pushed too far, and the picture of a liquid as surrounded by an elastic skin is a fiction which may on occasion prove harmful. The considerations just mentioned show that the surface is the seat of energy, and the concept of the *free energy* per unit area of the surface—that is the maximum work obtainable in an appropriate isothermal transformation—provides a very powerful weapon with which to attack surface problems. The concept of surface tension is, however, a very useful one, and we shall for the most part employ it in what follows.

We assume, then, that across a line of length ds drawn in a liquid surface there is exerted a force equal to γds, perpendicular to the line and in the tangent plane to the surface; if the surface is that separating the liquid from air, or from its saturated vapour, γ is termed *the* surface tension of the liquid.

A simple application of molecular theory, or an appeal to direct experience, shows that a liquid meets a solid at a definite angle, which may be acute or obtuse. For many liquids in contact with glass this contact-angle is zero.

Again, on the concave side of a contractile surface, there must be a positive pressure excess which may be shown to be equal to $\gamma(1/R_1 + 1/R_2)$, where R_1 and R_2 are the principal radii of curvature at the point at which the pressure excess exists. If the surface is part of a sphere, $R_1 = R_2 = R$, say, and the pressure excess becomes $2\gamma/R$. In the particular case where the surface is plane, $R_1 = R_2 = \infty$ and the pressure excess is zero.

Bearing this in mind, it is easy to see why, and to what height, a liquid having an acute, or zero, contact-angle with glass will rise in an open vertical capillary tube standing in a vessel containing the liquid.

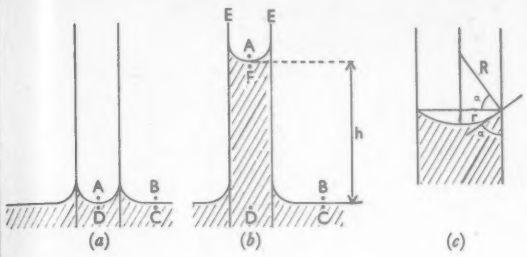


FIGURE 1

In figure 1 (*a*) we see an impossible position of equilibrium, for the liquid inside the capillary tube meets the glass at a constant angle α, and a curvature, which will be spherical if the bore of the tube is small enough, is imposed on the liquid within the tube. Now, since the surface of the liquid at C is plane we have:

Pressure at C = Pressure at B = Pressure at A
= Atmospheric pressure.

But, the meniscus in the tube being concave, the pressure at A exceeds the pressure at D by the amount $2\gamma/R$; so that we have two points, D and C, at the same level in a homogeneous liquid and at different pressures. This is impossible and the liquid must rise through a height h [figure 1 (*b*)] sufficient to produce a pressure ($g\rho h$) which will bring the pressure at D up to atmospheric value. Equating these expressions for the pressure excesses we have

$$2\gamma/R = g\rho h, \text{ or } \gamma = Rh\rho g/2$$

If the contact-angle is α and r is the radius of the tube, we have, from figure 1 (*c*):

$$r = R\cos\alpha, \text{ and } \gamma = rh\rho g/2\cos\alpha.$$

If the liquid wets the tube, $\alpha = 0$ and we have the well-known result

$$\gamma = rh\rho g/2.$$

All this is fairly obvious, but it serves to bring out the point that in the practice of the capillary-rise experiment the two quantities measured are a pressure excess ($g\rho h$) and a curvature ($2/R$), and the ratio of these two quantities determines the surface tension. The necessity for measuring the density of the liquid under test arises from the fact that the *liquid acts as its own manometer*. If, then, we measure the pressure excess by some other method, as, for example, by applying pressure at the upper end (EE) of the tube until the meniscus is forced down to the position shown in figure 1 (*a*), we can measure the pressure required on an independent (and possibly more sensitive) manometer, and a knowledge of the density of the liquid under test is not necessary.

Let us apply these principles to a specific problem. Given a small quantity of a liquid, say a cubic centimetre, or even so little as that rather vague quantity a minim or drop, is it possible to make a precision determination of its surface tension? Figure 2 shows a simple form of apparatus by which such measurements may be easily and rapidly made. The figure is more or less self-explanatory. The pressure-bottle and manometer on the right-hand side of the drawing are

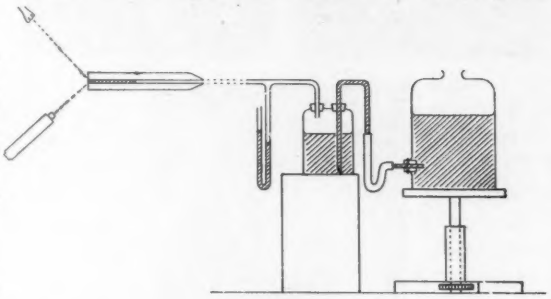


FIGURE 2

connected to a *horizontal* thick-walled capillary tube, of about a millimetre bore, which is shown on an enlarged scale on the left-hand side of the drawing. The end of this tube is ground flat to a matt finish and the short thread of the liquid under test is introduced into the tube. By raising the pressure-bottle shown on the extreme right of figure 2, the thread of liquid may be forced along the capillary until the meniscus at the open end is accurately *plane*. This position may be judged with very considerable accuracy by watching, through a low-power microscope or lens, the illumination of the meniscus provided by a 6-volt lamp. Figure 3 (*b*)–(*f*) shows a series of photographs of the end of the capillary: (*b*) and (*c*) show concave menisci, (*e*) and (*f*) convex menisci, and (*d*) shows the plane position in which the meniscus appears uniformly illuminated. The corresponding pressures read on an aniline manometer are shown in Table 1:

TABLE 1

Photograph	*b*	*c*	*d*	*e*	*f*
Pressure (cm. aniline) ..	2·689	2·836	2·887	2·955	3·108

It must not be assumed that these steps of pressure in any way represent the delicacy of the setting, which is very sensitive and clear-cut. The steps are shown at intervals of 0·5 mm. pressure-head or thereabouts to exhibit more clearly on the photographs the march of the

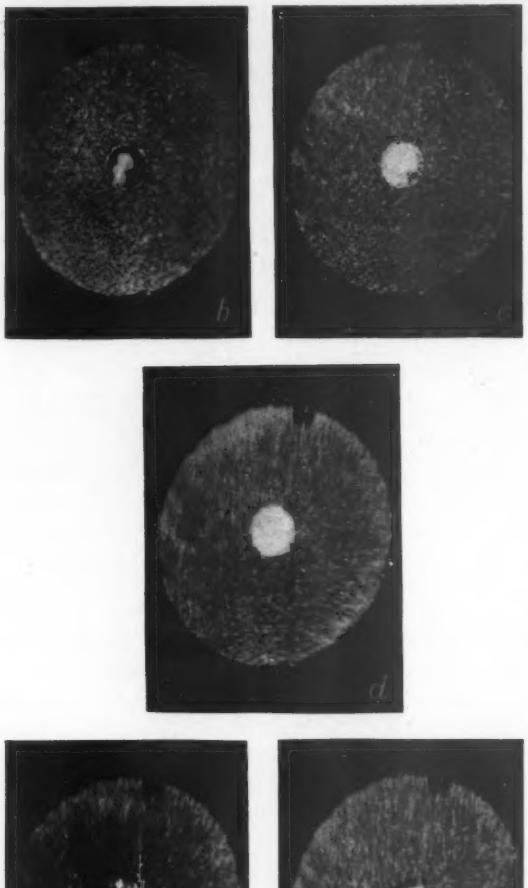


FIGURE 3

observed phenomena. It is obvious that the bore of the capillary must be small in order to ensure that there is no serious gravitational distortion of the meniscus—that is, to ensure that the meniscus may be assumed to be a segment of a sphere. This question has been fully investigated (Ferguson and Kennedy, 1932), and it has been shown that tubes up to 1 mm. bore may safely be used. It is clear from figure 4, which shows the horizontal capillary with the thread of liquid exhibiting a plane meniscus at the open end of the tube, that the pressures at D, C, and B are all equal and equal to the atmospheric pressure; that the pressure at A is therefore greater than atmospheric pressure by $2\gamma/r$, where if we assume a zero contact-angle the meniscus is hemispherical, and r is to be identified with the radius of the tube. But (*see* figure 2) the pressure at A is also seen to be greater than atmospheric pressure by $g\rho h$ where h is the pressure-head read on the manometer and ρ the density of the manometer liquid. Hence we have

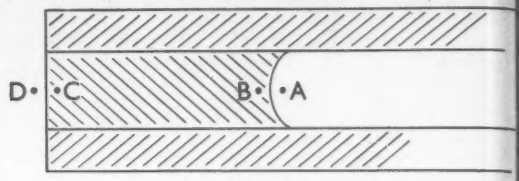


FIGURE 4

$$2\gamma/r = g\rho h \text{ or } \gamma = rh\rho g/2$$

We have assumed a zero contact-angle, and in fact research has shown that a zero contact-angle with glass exists for many organic liquids. The relevant article in International Critical Tables (1928) lists a small number of liquids for which this has been proved directly, and proceeds: 'That α for liquid-to-glass is zero . . . is indicated in the case of about 100 other liquids by the data used in compiling these tables, which show that the surface tension determined by the capillary height method on the basis of the assumption that α is zero is the same as the value obtained by the drop-weight method or by the bubble-pressure method.' Since these last-named methods are independent of the contact-angle, this is unimpeachable evidence.

It is important to be able to make measurements of the variation of surface tension with temperature, and a convenient arrangement for the purpose is shown in figure 5. This represents a simple form of furnace consisting of a brass tube 10 cm. long and 3 cm. in diameter overwound with nichrome wire. The furnace was closed with corks bored to accept the horizontal capillary which projected from the furnace at both ends. Temperatures were read by a chromel-alumel thermocouple, and for measurements over a temperature range of, say, 15° to 70° C. this arrangement of the tube was adopted. For substances which melt at a fairly high temperature

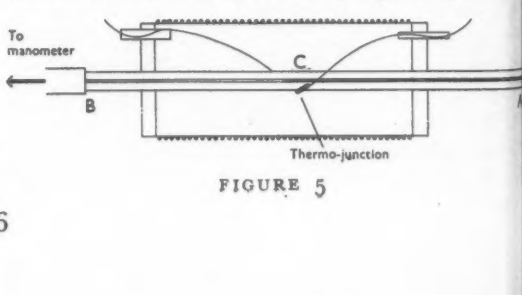


FIGURE 5

(e.g. 100° C.) the open end of the capillary was contained within the furnace, and the corresponding end of the furnace closed with a plate of optical glass through which the planeness of the meniscus at the end of the capillary within the furnace could be conveniently observed. Figure 6 shows the arrangement adopted for the measurement of the surface tensions of water and of heavy water.

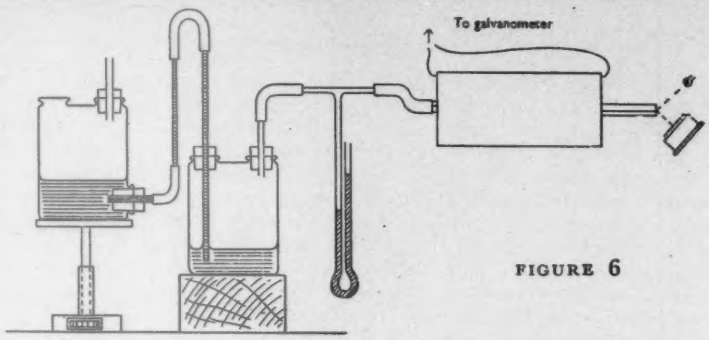


FIGURE 6

And now, having described a practicable and simple apparatus, we may ask: What results have been obtained by its use and what new results is it profitable to seek?

In the first place, it may be said at once that, despite the thousands of measurements that have been made, there are many and serious gaps in the literature, and that many of the measurements listed in tables have small value, consisting, as they do, of records of measurements made at one or two temperatures on isolated, or unrelated, substances. Anyone who will undertake the measurement of the relation between surface tension and temperature for the members of a homologous series of carefully purified organic compounds will provide data of great practical and theoretical value.

It may be remarked here that the practice of representing surface tension–temperature results by such a formula as $\gamma = \gamma_0 (1 - at)$ is to be deprecated. The formula will not bear extrapolation. Remembering that surface tension vanishes at the critical temperature, we see that, if such a formula is valid, $t_c = 1/a$, and values of t_c calculated in this way are wildly erroneous. van der Waals (1894) proposed a formula $\gamma = \gamma_0 (1 - bt)^n$, which passed unnoticed and was rediscovered independently by the writer of this article (Ferguson, 1916). This formula, which is valid for unassociated liquids only, represents the relation between surface tension and temperature over the whole range of existence of the liquid, from freezing-point to critical temperature. To a first approximation the value of n is the same for all liquids and may be taken as equal to 1·2. If the highest accuracy is desired, account must be taken of the small variations of n from liquid to liquid. If the formula be written $\gamma = \gamma_0 (1 - b\theta)^n$, where θ stands for the absolute temperature (t representing Centigrade temperature), then γ_0 stands for the free surface energy of the supercooled liquid at absolute zero and is an important constant of the liquid. The variation of its value for the different members of a homologous series is a property the study of which demands investigation. The quantity b in either formula is accurately the reciprocal of the critical temperature, and it becomes possible to determine the critical temperature of an unassociated liquid from observations of its surface tension made over a relatively restricted range of temperature, after determining the constants γ_0, b and n in the formulae just cited. The accuracy with which the critical temperature is represented by $1/b$ is shown in Table 2:

TABLE 2

Substance	n	b	$t_c = \frac{1}{b}$	t_c–obs.	Diff.
			°	°	°
Ether	1·248	·005155	194	193·8	+0·2
Benzene	1·218	·003472	288	288·5	−0·5
Chlorobenzene	1·203	·002793	358	359·2	−1·2
Carbon tetrachloride ..	1·206	·003553	281·5	283·1	−1·6
Methyl formate	1·210	·004695	213	214·0	−1·0
Methyl acetate	1·200	·004274	234	233·7	+0·3
Methyl propionate ..	1·202	·003891	257	257·4	−0·4
Methyl butyrate.. ..	1·195	·003559	281	281·3	−0·3
Methyl isobutyrate ..	1.228	·003731	268	267·6	+0·4
Ethyl formate	1·187	·004255	235	235·3	−0·3
Ethyl acetate	1·217	·003984	251	250·1	+0·9
Ethyl propionate ..	1·192	·003663	273	272·9	+0·1
Propyl formate	1·231	·003774	265	264·9	+0·1
Propyl acetate	1·204	·003623	276	276·2	−0·2

Mean value of n = 1,210.

For associated liquids no such formula holds and it is difficult to devise a formula which will bear extrapolation. Through the kindness of Imperial Chemical Industries the writer was some time ago supplied with a sample of heavy water of 99·2 per cent. purity, for the purpose of determining its specific heat, and a determination of its surface tension was also made (Cockett and Ferguson, 1939). Some previous observers had reported a very low surface tension for deuterium oxide, viz. 67·8 dynes per centimetre at

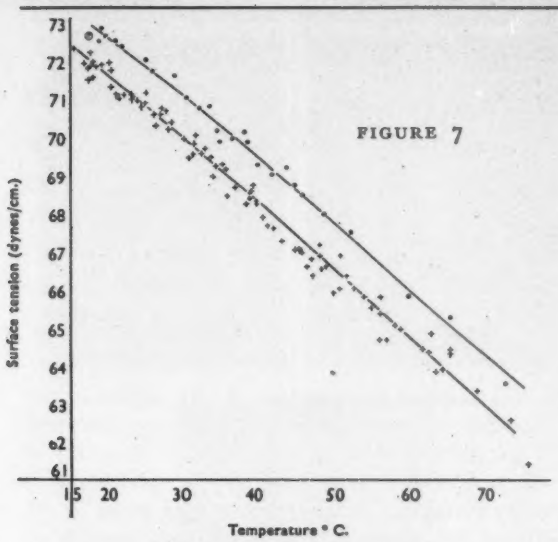


FIGURE 7

20° C. as compared with 72·8 dynes per centimetre for the surface tension of ordinary water at this temperature. Other observers had found no appreciable difference between the surface tensions of ordinary and heavy water. The problem was investigated by means of the apparatus shown in figure 6. The results obtained may be summed up in equations of the form $\gamma = A - Bt^n$, where, for ordinary water, A = 75·23, B = 0·0667, $n = 1{\cdot}20$; for heavy water, A = 74·64, B = 0·1082, $n = 1{\cdot}10$. The results obtained, and the agreement between the observed and calculated values, are shown in Table 3. Figure 7, which also shows the scatter of the observations, exhibits the differences graphically.

TABLE 3

Heavy Water				Ordinary Water			
t	γ obs.	γ calc.	Diff.	t	γ obs.	γ calc.	Diff.
° C.				° C.			
12·1	72·88	72·96	− 0·08	17·3	72·84	73·19	− 0·35
15·2	72·40	72·48	− 0·08	20·4	72·75	72·74	+ 0·01
20·1	71·97	71·70	+ 0·27	25·5	71·94	71·98	− 0·04
24·0	71·50	71·07	− 0·02	28·6	71·86	71·50	+ 0·36
30·1	70·17	70·06	+ 0·11	30·1	71·20	71·27	− 0·07
35·0	69·51	69·24	+ 0·27	34·5	70·35	70·56	− 0·21
39·5	68·44	68·47	− 0·03	38·4	70·07	69·92	+ 0·15
45·0	67·22	67·51	− 0·29	44·6	68·86	68·88	− 0·02
50·8	66·18	66·50	− 0·32	50·0	68·13	67·94	+ 0·19
55·0	65·63	65·75	− 0·12	60·2	65·93	66·11	− 0·18
61·9	64·54	65·52	+ 0·02	65·7	65·39	65·11	+ 0·28
64·5	63·99	64·05	− 0·06	73·1	63·60	63·99	− 0·39
69·0	63·37	63·24	+ 0·13				
73·5	62·66	62·42	+ 0·24				

It has been remarked that for substances which melt at relatively high temperatures the arrangement of the furnace and tube must be modified as shown in figure 8. This arrangement has been

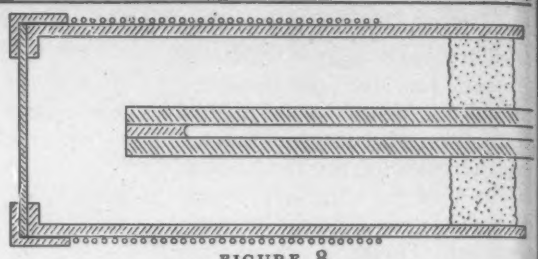


FIGURE 8

used (Ferguson and Kennedy, 1938) to study the interesting variations of surface tension with temperature which occur in the case of liquid crystals—substances that melt to form doubly refracting liquids which on further heating lose these doubly refracting properties at a definite transition temperature. Such substances are anisaldazine, *p*-azoxyanisol and *p*-azoxyphenetol, all

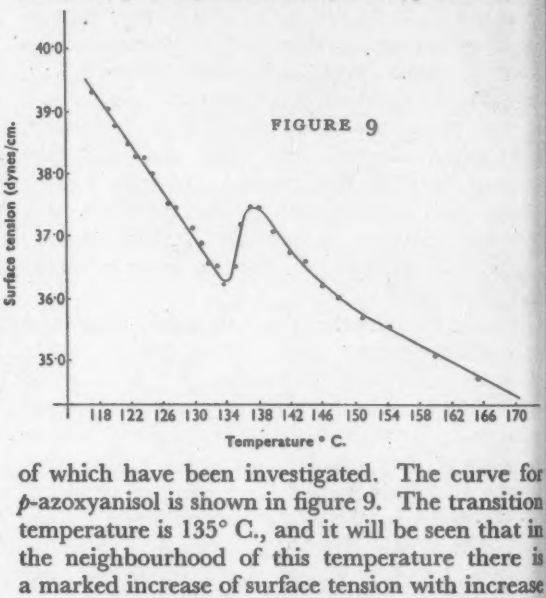


FIGURE 9

of which have been investigated. The curve for *p*-azoxyanisol is shown in figure 9. The transition temperature is 135° C., and it will be seen that in the neighbourhood of this temperature there is a marked increase of surface tension with increase of temperature. This remarkable phenomenon is exhibited by all the liquid crystals examined and demands theoretical investigation.

Applications could be multiplied *ad libitum;* but this article will have served its purpose if it has shown that a wide field of research exists in which most useful results may be obtained by relatively simple apparatus and technique.

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Book reviews

THE PRESENT POSITION OF EVOLUTIONARY THEORY

Evolution: the Modern Synthesis, *by J. S. Huxley. Pp.* 645. *George Allen and Unwin Limited, London.* 1942. 25*s. net.*

This is a very important book—important for being exactly what its author claims: a synthetic account of the modern view of the most important of biological problems. It comes at a very interesting time in the development of our understanding. Until about twenty years ago, our knowledge of the fundamental processes of evolution remained almost where it had been in Darwin's time. The only 'advance' had centred round the emphasis on the half-truth of discontinuity in variation and the theoretical application of an over-simplified Mendelism. The true implications of the new knowledge of heredity were, perhaps, first glimpsed by Tchetverikov, who, shortly after the first phase of the great Russian revolution, pointed out that in animal populations too the full potentialities do not lie open to inspection at the surface but may exist in a hidden, recessive form for many generations until, following some change in circumstances, they become advantageous and spread through the whole species. This line of thought initiated two active schools of work—the theoretical investigations of Fisher, Haldane, and Wright, and the practical investigation of natural populations by Dobzhansky, Timoféef-Ressovsky, Dubinin, Gordon, and others. Meanwhile, geneticists such as Stern and Muller were developing the idea of the 'genotypic milieu' and the mutual interaction of genes; the evolutionary bearings of their ideas were incorporated by Fisher and Wright into their mathematical treatment of natural selection. Another strand to be woven into the fabric came from the investigations of systematists into the geographical variation of animals; stimulating, and itself stimulated by, the mathematical treatment of Wright, it issued in the most important single new theory, that of the 'drift' of populations under the guidance, not of natural selection, but of chance.

In the last twenty years, then, several new avenues have opened up and the first steps have been taken along each of them. Huxley, with his very wide reading and a true insight into the real nature of the advance, fits into the framework of his synthesis a richer accumulation of facts than is found in the only other comparable book in English, Dobzhansky's *Genetics and the Origin of Species*. He is particularly successful in his treatment of systematic and ecological data, which are often too little known by geneticists. And he has not only a story to tell of past achievements, but many doors to open towards the future. It is only the first crop which has yet been taken off the fields spoken of above—there is much to do before we need think of leaving them fallow. And there are fields even more recently surveyed—the problems, discussed by Darlington, of the evolution of genetic systems themselves; and the old, but now perhaps rather less intractable problems, recently the subject of a book by Goldschmidt, of the limitations imposed by the developmental system on the variation available for evolution. Huxley's book is, in fact, something more than a synthesis in the chemical sense which implies the achievement of a fixed and determined goal; the unity which it records is a stage in an ever-deepening and developing knowledge.

C. H. WADDINGTON

ANIMAL INVADERS

Voles, Mice, and Lemmings, *by Charles Elton. Pp.* 496. *The Clarendon Press, Oxford.* 1942. 30*s. net.*

Overwhelming plagues of voles and devouring hordes of migrating lemmings—these are occurrences which have impressed observers from earliest historical times. They have often spelt ruin for the farmer, and have even forced their attention upon disinterested governments. In themselves they constitute a formidable challenge to the scientist, a challenge which is fully answered in this authoritative book.

Too often has it been supposed in the past that these mighty pulsations of nature are 'unnatural,' so quick is the human tendency to forget bygone disasters. In this book such prolific increases are placed in their proper perspective to the normal ebb and flow of animal, and particularly rodent, populations. The periodic fluctuations in the numbers of these small herbivores have an inevitable effect upon the abundance or scarcity of the many predators who, directly or indirectly, benefit from them. This is a theme which is followed out in a fascinating manner throughout the book.

Part I is concerned with the history of vole plagues in Central Europe and the means that have been used to combat them. Part II deals with rodent fluctuations in North-west Europe, including the famous lemmings of Scandinavia. In this section are also two important chapters on the new methods developed at Oxford to study the density of rodent populations. It might be said that these two parts illustrate in a very graphic manner the way in which animal ecology as an exact science is growing out of natural history.

Parts III and IV contain the most unusual feature of this book—the detailed analysis of wild-life cycles in Labrador and Ungava, derived from hitherto unpublished material in the trade archives of the Moravian Missions and the Hudson's Bay Company. Periodic oscillations, which had been demonstrated experimentally for the Oxford voles, stand out with almost startling clarity for some of the Labrador predators, which, in turn, were shown to depend on rodent cycles. Particularly noteworthy are the four-yearly cycle of abundance of the Labrador foxes and the ingenious methods Mr Elton has used to show that the fur returns are a reliable index of population density.

The original question as to the cause of these prodigious fluctuations in rodent numbers remains partially unanswered, but some promising lines of research are discussed.

It is hardly possible in the space of a short review to refer even in brief to the wealth of facts mustered in this book, nor is it possible to convey the vivid style in which it is written, a style enhanced by the author's inclusion of many contemporary notes and descriptions.

A. D. LEES

EXPLOSIVES FOR THE MILLION

Explosives: Tells of explosives, their magical creation, their fierce energy, their sudden disruption, their history and romance, and their uses in Peace and War, *by John Read. Pelican Series,* 1942. 9*d.*

Professor Read has found full scope for his skill as a dramatist, teacher, organic chemist, and historian in this book on explosives in the Pelican series. He develops the theme of the sun's energy, which, converted into organic energy in the leaves of plants, is by-passed into explosives by various

arts developed by man. Nature in her wisdom 'has refrained from including in her lavish chemical cornucopia any substance of an explosive nature,' but Nature's wilful child, like unto a 'Maxwell's demon,' has sorted out those bodies which can be prompted to release their energy with cataclysmic violence. The organic chemist, by providing buffers of nitrogen atoms, has introduced the combustible and the supporter of combustion into the same molecule, which makes possible a speed of combustion limited only by the velocities of elastic waves. Derived in the main from carbohydrates, fats, and coal, the nitro-bodies form the principal classes of explosives, but rarely yield more than 50 per cent. of the energy available in the parent substances and atmospheric oxygen. Thus, although the chemical bond groans under the strain imposed upon it, the organic chemist has still worlds to conquer.

The inorganic chemist has not been idle, for he has provided the 'fifth columnists' of the explosive world, azides and fulminates, which are employed to release the energy conserved by the organic chemist. He can claim most of the incendiaries and might have claimed the first explosive of all, except that in the thirteenth century alchemy had not yet been differentiated into its component parts. It is now exactly 700 years since Roger Bacon 'made known' the composition of gunpowder. This anniversary is fittingly celebrated by a historical chapter on gunpowder from the author's pen.

This is a book which combines the elements of a textbook and a novel. It is a story of the romance of chemistry that does not burke an account of its shorthand, its terminology and its method of formulation, which are explained to the reader in imagery with which he is familiar from practical experience. It is opportune, and practical in its outlook. It is not confined to the chemistry of explosives, but describes and illustrates methods of manufacture and assembly. The expert will find facts in it that are new to him, and the Home Guard much that will explain the vagaries of the weapons entrusted to his inexperience. The latter may even learn of the means to increase his arsenal, although warned by the author of possible fatal consequenecs.

It is sad to reflect that, but for the requirements of nitrocellulose for war purposes, this might truly have been a book for the million.

W. E. GARNER

CELLULOSE

An Introduction to the Chemistry of Cellulose, *by J. T. Marsh and F. C. Wood. Pp. xv* + 512, *with* 24 *plates and* 145 *figures. Chapman and Hall Limited, London. Second edition,* 1942. 28*s. net.*

The chemistry of cellulose and cellulose derivatives is both an extensive and a specialized study. It is also a study in which very remarkable progress has been made within the last few years. Quite apart from its technical importance, it has a general interest for the organic chemist, the biochemist, and the biologist. From these facts the need for an authoritative introduction to the subject becomes apparent, and when the present book was first published, in 1938, it was deservedly welcomed. The authors have a probably unrivalled knowledge of cellulose chemistry, pure and applied, while their familiarity with the relevant literature appears to range from A to Z without a gap. Though the book is modestly described as introductory, and though in fact it does start *ab initio*, any reader who masters it will have acquired a comprehensive working acquaintance with the whole field. A pleasing—and valuable—feature of the book is the readable style in which it is written. In this new edition the latest developments in cellulose chemistry have been incorporated; otherwise it closely follows the lines of the successful original edition of 1938.

RADIO COMMUNICATION

Short-wave Wireless Communication, *by A. W. Ladner and C. R. Stoner. Pp.* 573. *Chapman and Hall Limited, London. Fourth edition,* 1942. 35*s. net.*

Every radio engineer and physicist knows 'Ladner and Stoner,' in the form of one of the earlier editions, and to them the publication of a fourth edition is a matter of the greatest interest. The contents are restricted to the title-subject of 'short-wave wireless *communication*' in its narrowest sense, and there is no mention of some of the most interesting applications of short-wave techniques, such as aids to aerial navigation and blind landing, direction and position finding, and television aerials, even in their well-known pre-wa aspects. In the communication fiel one notices the absence of any dis cussion of the very interesting system of frequency modulation now i use in America. Curiously enough although these matters of grea interest are neglected, a chapter o twenty pages is devoted to 'high frequency therapeutic apparatus.'

In general, the treatment of th circuit problems of high-powere oscillators, modulators and amplifiers which form a large part of the book is full and satisfying; while the ac counts of transmission lines, aerials and propagation are full but rathe superficial, since little attempt i made to explain the underlyin physical principles. The account of th super-regenerative receiver appears t be erroneous.

In summarizing, it may be sai that the book is a very good and, o the whole, readable account of th subject as seen from the limited poin of view of the Marconi Wireless Tele graph Company; but to the radi physicist in particular that point o view appears to be an unnecessar limitation of the title 'Short-wav Wireless Communication.'

J. A. RATCLIFF

THE SCIENTIST AND THE STATE

The Scientific Life, *by J. R. Baker Pp.* 154. *George Allen and Unwi Limited, London.* 1942. 7*s.* 6*d. net.*

In this stimulating and provocativ book Dr Baker throws down th gauntlet to those men of science wh do not conceal their admiration o totalitarianism and would like to se science organized under state contro He is able to show, in a manner tha most readers will find convincing, tha science flourishes best in an atmosphe of freedom; that most of the greate scientific discoveries have been mad by individuals and not by teams o groups of workers; and that there is very real danger of science becomin fettered by bureaucratic control, pr fessedly for her own good. But D Baker has made the tactical error o overstating a sound case and has thu unnecessarily laid himself open t grave criticism upon several point In general, however, the book is bot welcome and timely; Dr Baker wield a stout lance, and if he sometimes til at a windmill it may be because h has reason to suspect camouflage.

Notes on contributors

SIR JAMES JEANS,
O.M., F.R.S.,

Was born in 1877 and was educated at Merchant Taylors' School, London, and Trinity College, Cambridge. From 1905 to 1909 he was Professor of Applied Mathematics at Princeton University, having been appointed by Woodrow Wilson at the time when a closer approximation to English methods and atmosphere was being introduced into the University. Later he became Stokes Lecturer in Applied Mathematics in the University of Cambridge, and is now Professor of Astronomy in the Royal Institution. Since 1923 he has been a Research Associate of the Mount Wilson Observatory, California. On Lord Rutherford's death in 1937 he was appointed at short notice to take his place, first as leader of the British Association delegation to India, and second as president of the Jubilee Meeting of the Indian Science Congress Association. He has served on the Advisory Council of the Department of Scientific and Industrial Research and is a member of many learned bodies, both British and foreign. Among his numerous distinctions are the Royal Medal of the Royal Society (1919), the Gold Medal of the Royal Astronomical Society (1922), the Franklin Medal of the Franklin Institute (1931), and the Calcutta Medal of the Royal Asiatic Society of Bengal (1938). To the general public he is well known as one of the most lucid expositors of astronomy and cosmology.

Apart from science, his main interest is in music, and he is at present a Director of the Royal Academy of Music. His own instruments are the organ and piano. He is also a keen gardener.

COLONEL SIR HENRY LYONS,
Hon. D.Sc. (Oxon.), Hon. Sc.D. (Dublin), F.R.S.

The son of General T. C. Lyons, C.B. (Governor of Bermuda, 1892–96), he was born in 1864 and was educated at Wellington College and the Royal Military Academy. Commissioned in the Royal Engineers in 1884, he subsequently served at Gibraltar and in Egypt. He was Director-General of the Geological Survey, Egypt, from 1896 to 1898, and of the Survey Department of that country from 1898 to 1909. After his retirement from the Egyptian Service he was appointed to the staff of the Science Museum, of which he was made Director in 1920. He was Foreign Secretary of the Royal Society in 1928–29 and Treasurer from 1929 to 1939. He has made an exhaustive study of the history of the Royal Society, particularly in its administrative aspect.

K. MATHER,
D.Sc., Ph.D.,

Was born in 1911, and was educated at Nantwich and Acton Grammar School and the University of Manchester. He has carried out research upon plant breeding at Svalöf (Sweden), was Rockefeller Fellow at the California Institute of Technology and at Harvard University in 1937–38, and since then has been Head of the Genetics Department of the John Innes Horticultural Institution. Mainly concerned with the genetics of those quantitative or 'polygenic' characters basic to evolutionary theory and to the practice of plant and animal improvement, he has also written books on statistics and believes that all students of biology should be taught statistical analysis as an essential part of their training in experimental science.

L. LEVY,
M.A., D.Sc., F.I.C.,

Was born in 1885 and educated at University College School, London, and Clare College, Cambridge. For the last three years of the war of 1914–18 he served in the Royal Engineers Anti-gas Establishment of the Army, retiring with the rank of brevet-major. Shortly afterwards, he and Mr D. W. West (*see next column*) started together in practice as consulting and research chemists, specializing in the development and use of luminescent substances for all purposes. They are the joint inventors of an X-ray fluoroscopic screen which is employed in all parts of the world for diagnostic work. They have also concerned themselves with the development of luminescent materials for cathode-ray tubes for television, for illumination, and for the production of radio-active luminous substances. They have collaborated in writing many original articles and papers on the practical use of luminescent materials.

D. W. WEST,
A.C.G.I., A.I.C.,

Was born in 1894 and was one of the last of H. E. Armstrong's students in the chemical department of the City and Guilds Engineering College, London. He served in the Army in the last war, first in the Special Brigade of the Royal Engineers and later in the Royal Engineers Anti-gas Establishment. (*See also under* L. LEVY.)

ALEXANDER HADDOW,
M.D., D.Sc., Ph.D.

Born in 1907 at Leven in the 'Kingdom' of Fife. Educated at Broxburn High School and the University of Edinburgh, where he graduated in 1929 and later became Lecturer in Bacteriology (1931–36). During this time published a number of papers on special aspects of the experimental pathology of growth (mainly problems of immunity and the principles governthe transplantation of tissues) and on the phenomena of 'dissociation' in bacteria. In 1935 discovered a growth-inhibitory property characteristic of carcinogenic compounds and has spent some years—since 1937 with Kennaway at the Chester Beatty Research Institute of the Royal Cancer Hospital—in developing this subject and relating it to the mechanism of action of tumour-producing agents generally.

ALLAN FERGUSON,
M.A., D.Sc.,

Is a Lancastrian, having been born at the moorland village of Entwistle, near Bolton-le-Moors, in 1880. He was educated at the Harris Institute, Preston, and the University College of North Wales, Bangor. At an early stage of his career he deserted pharmacy for physics, and is now Assistant Professor of Physics at Queen Mary College in the University of London. He is one of the general secretaries of the British Association for the Advancement of Science, a past president of the Physical Society, and one of the editors of the *Philosophical Magazine*. He has written many papers on capillarity and on the determination of specific heats. A collector of editions of Horace, Johnsoniana, and contemporary reports of trials, he is interested in dialects, likes gardens (but dislikes gardening), and is not a golfer.

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